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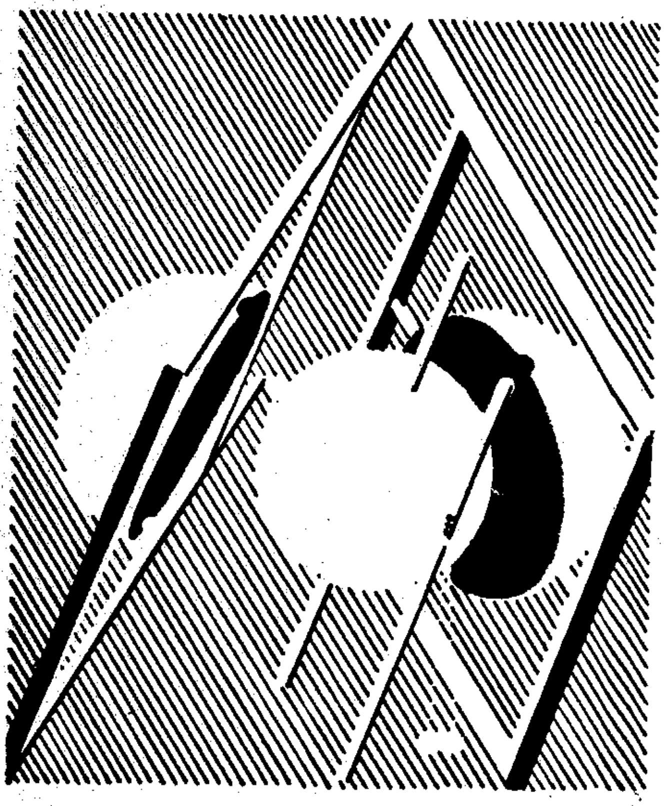
#13

HENRY CAVENDISH

# The Law of Force of Electricity



History of Physics Laboratory
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# Henry Cavendish

## The Law of Force of Electricity




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## Synopsis:

Henry Cavendish's was the first comprehensive theory of electricity; and his experiments and measurements--although not fully published in his own lifetime--were by far the most significant attempt to explore, quantitatively, the phenomena and to analyze the results in terms of a detailed theory.

Natural Science in the eighteenth century had reached a stage where the interests of the mathematical philosophers (precision and analysis) and those of the experimental philosophers (exploration of new phenomena) had become separate and even divergent. Electricity, in particular, was a domain largely for the experimentalists, many of them amateurs.

Henry Cavendish--an eccentric genius of high birth--was one of the very few who was able to bring to the study of electricity (and to many other scientific subjects) gifts which embraced the mathematical, analytical, and experimental; and a rare passion for observation of natural phenomena of every variety.

His determination of the law of force between electric charges is a model of scientific experimentation. It was, and still is, the basis of a whole series of subsequent experiments, extending from 1871 to the present day. With up-to-date techniques, the experiment can be performed with extraordinary sensitivity; and there is still an interest in ever-higher precision, in probing the limits of the range over which the validity of the law-of-inverse-squares can be tested, experimentally.

## I. Preface. 18th Century Natural Philosophy

There is a familiar picture of the 18th Century: The Age of Reason and Enlightenment; an age of confident, unhurried Progress, of restraint, discrimination and urbanity. In this setting, Natural Science, reflecting the character and coloration of the period, is portrayed as a steady if unspectacular development, a sort of interregnum between the seminal works and brilliant accomplishments of 16th and 17th centuries, and the exuberant expansion of science in the 19th.

It may seem rather ingenuous to assign a particular character to the culture or science of a century, as if historical development can be cut into regular 100-year slices, each synchronized neatly with the calendar. Yet there is an unmistakable "18th-century," which imparts its character to much science from 1700 to 1800. It is a period whose beginning is marked by the appearance and immense influence of Newton's masterpieces: the Principia and Opticks; it ends with the discoveries of Alessandro Volta, Humphrey Davy, and Thomas Young; and the work of Joseph LaGrange and Pierre S. de Laplace; it extends from the death of Boyle, Huygens, Leibnitz, to the birth of Gauss, Fresnel, Faraday, and Carnot. From the time of Queen Anne and Louis Quattorze to that of Jefferson and Napoleon may seem like an epochal separation, yet much of the intervening period has a character and constancy that seems to belie change; that suggest that development if not lacking is, at least, latent.

The 18th Century is not only an age of reason, but of secularism, tolerance and humanism, whose expression requires a measure of constancy and stability in both knowledge and society. It is the great age of the philosophers and encyclopedists,--and how can one organize, codify, assimilate, and disseminate knowledge if its basis is shifting dramatically? In the Sciences, the spectacular achievements of the natural philosophers of the seventeenth century had endowed their successors with so splendid a patrimony, that to seek new scientific fortunes and adventures must have seemed to many, if not impossible, at least inopportune. What better task than to husband and develop the fortune at hand, and to reap its benefits? Why attempt to build anew, when such splendid foundations have been inherited?

And so, with diligence, application and the characteristic genius of the age, did the Bernoullis, Euler, D'Alembert, La Grange, LaPlace and many others labor to formalize, refine, extend and embellish the edifice of Mathematical Natural Philosophy in the spirit of its great architect, Isaac Newton.

But the 17th century is noteworthy not only for what it accomplished, but also for what it initiated. It founded, and bequeathed to successors, the numerous scientific societies and Academies where men could cooperatively indulge in their individual curiosity about Nature, and where this interest could be sustained by continuing, if not always systematic experimentation and discussion. Study of the workings of nature in this way was accessible to many who possessed neither the scholarship nor the mathematical skill demanded of those who would contribute to formal Natural Philosophy. "Nature" had become very fashionable by the 18th century; and there was mathematical philosophy to probe its profundities, and experiment to explore its innumerable shapes and forms. Neither of these was the creation of the 18th century, but in it both were firmly established and extensively exploited.

Most of those who contributed to Natural Sciences in the 18th century chose one approach or the other--the mathematical or the experimental; few had the aptitude or ability to embrace, still less to excel in, both. "Nature" covered an immense variety of phenomena. Experiment and observation were still sufficiently unsophisticated for an amateur with little specialized knowledge and even less preliminary training, readily to enter into the fray (as many did). And their labors resulted in a great accumulation of empirical knowledge (and no small speculation) in chemistry, geology, zoology, botany, astronomy and those branches of physics still too empirical to be assimilated into the Newtonian framework, e.g. Acoustics, Metallurgy, and Electricity. There were outstanding geniuses here also: Buffon, Franklin, Priestley, Lavoisier, Herschel, Rumford, Watt; men typically less professionally academic than their mathematical-philosophical counterparts, who if not strictly amateurs, were men of many parts, of which science was one. Paradoxically, the genius of the eighteenth century was often its many-sidedness; there were those whose interests and activities spanned the whole range of science and public affairs. Philosophy, science, government and politics were in a sense as close together as they ever came. Yet within natural philosophy itself, there was a marked dichotomy between the formal-analytical, and the exploratory-experimental.

The study of electricity in the eighteenth century was predominantly of the latter category, at least at the outset. For most of the century the problem was to identify and clarify the phenomena, to observe carefully and unravel patiently the seeming endless complexities, so as to bring electricity within the range of precise formulation and then formal analysis.* By the end of the century the science of "electrostatics" at least was approaching this latter stage, but few in this whole period of development possessed the power to embrace the whole range of theory and experiment and attempt a comprehensive view.

One exceptional genius, Henry Cavendish, who had both the power and the inclination to attempt, and in a large measure succeed in, this task, ironically, had only a limited influence on the development of electrical science. He is celebrated today (in physics) for two major achievements: his elucidations of the law of force (the "inverse square law") in electricity, and the determination of the Universal Gravitational Constant (G). In retrospect, we see these as two different but both profoundly fundamental problems necessary for the full development of the Newtonian scheme of natural philosophy. The accurate and thorough study of these

*Leonhard Euler, the great mathematical genius of the 18th Century virtually gave up. In his celebrated letters to a German Princess (1761) he writes: "The subject...(electricity)... almost terrifies me. The variety it presents is immense, and the ennumeration of its parts serves rather to confound than to inform--almost every day (there is) discovered some new phenomenon...the fatigue of wading through diffuse, long, tedious detail..."

two major forces of Nature--gravitation and electricity--would seem to us, as a natural and primary focus of scientific endeavor in any attempt to extend and explore the ramifications of Newtonian science. Yet the combination of analytical and experimental power required to resolve these problems was so rarely combined in a single individual, that it fell to this eccentric genius of the 18th century to deal successfully with them both. Kepler, Huygens, and Newton had shown how the law of inverse squares manifested itself in the realm of heavenly motion; it was Henry Cavendish who first demonstrated how this law was expressed in the more complex, but experimentally more accessible, domain of terrestial-laboratory phenomena.

*H. Cavendish*

THE HONOURABLE HENRY CAVENDISH,

*Born 10th Octr 1731 Died 24th February 1810.*

*(From a Drawing by Alexander in the Print Room of the British Museum)*

John Weale

## II. The Honorable Henry Cavendish (1731-1810)

Henry Cavendish is more than an 18th century character--he is almost a caricature of the period and the society into which he was born. He was "high-born"--a scion of the illustrious house of Cavendish which could trace its lineage back eight centuries to Norman times. It was already famous in the time of William the Conqueror. Amongst his ancestors was one Sir John Cavendish, Lord Chief Justice in the reign of Edward III (1312-1377), and later beheaded by Richard II; another, Sir William Cavendish, was treasurer to Edward VI (1537-1553), and it was he who established the family estates which were to be the basis of the immense wealth of the Cavendishes. By judicious marriages and alliances, the house Cavendish spread and gained influence: and during the reign of James I, a Cavendish was created the first Earl of Devonshire. Another of Cavendish's ancestors, Thomas Cavendish, was one of the first to circumnavigate the globe (1586/88), and perished, in Brazil, in a subsequent voyage of exploration.

The third Earl of Devonshire (1640-1707) was the most politically renowned. Opposed to what he regarded as the political repressions of James II, he took a prominent part in the accession of William (III) and Mary to the English throne, and was rewarded, in turn, by being created the first Duke of Devonshire. Henry Cavendish was his great-grandson: the eldest son of the third son of the second duke.

Henry Cavendish's own father--Lord Charles Cavendish--was himself a scientist of some attainments: he had participated in many of the electrical researches of the Royal Society in the 1740's, and had later become its vice-president. The education he arranged for his eldest son appears to have been in the best upper-class tradition of the time. He entered St. Peter's College at Cambridge in 1749, and remained there until 1753. He left without taking a degree, but the records indicate that this should not be interpreted as a measure of his intellectual attainments at Cambridge.

Little definite is known about Cavendish's life for the decade or so after leaving Cambridge, but it seems almost certain that he returned to London--to his father's house in Marlboro Street, and became immersed in scientific researches. Scientific investigations were already a familiar part of the scene in this aristocratic household: Lord Charles Cavendish's experimental activities and his interest in electricity, capillarity, and allied subjects were sustained until his death in 1783.

Henry Cavendish's own researches appear to have been pursued quite independently. Whilst his financial circumstances allowed him full personal freedom to pursue his interests, it seems that he did not have access to the great family wealth until after his father's death. A contemporary (Dr. Thomas Thomson) writes of him:

> "During his father's life time he was kept in rather narrow circumstances, being allowed an annuity of £500 only, while his apartments were a set of stables fitted up for his accommodation. It was during this period that he acquired those habits of economy and those singular oddities of character which he exhibited ever after in so striking a manner. At his father's death he was left a very considerable fortune; and an aunt, who died at a later period bequeathed him a very handsome addition to it, but in consequence of the habits of economy which he had acquired, it was not in his power to spend the greater part of his income." (Ref. 1, p. 159)

There are other recorded versions of how Cavendish acquired his own substantial fortune--that an uncle abroad, shocked at his family's neglect of a favorite nephew, left him a legacy of £ 300,000 in 1773. But in any event for the first 40 years of his life, financial factors had little influence on his devotion to science. He was free to pursue his interests as a dedicated "amateur"; but he was not subject to much temptation by way of great riches. From all we know of his character, it is doubtful whether these would, even if accessible to him, have deflected his purpose. Later in life, when in control of abundant wealth, the only objects on which he lavished it were scientific apparatus and books and accommodations to house them.

Cavendish did not publish any of his researches during the thirteen years after leaving Cambridge, but there is little doubt that in much of this period he was studying and experimenting, particularly in chemistry and electricity. In 1760 he was elected Fellow of the Royal Society, and his first paper, "On Factitious Airs" was published in 1766. A few years later, in 1771, he published his major work in electricity, modestly entitled:

> <u>An Attempt to Explain Some of the Principal Phenomena of Electricity by Means of an Elastic Fluid</u>
>
> (Phil. Trans. of the Royal Society, Vol. 61, pp. 584-677.)

This work, and his remarkable paper of 1775: <u>An Account of Some Attempts to Imitate the Effects of the Torpedo by Electricity</u>, (Phil. Trans. of the R.S. Vol. 66, pp. 196-225), are the only electrical researches of his which were published during his lifetime. Both papers, however, reveal such mastery of the subject, in so many respects so far ahead of his contemporaries, that it is not difficult to infer the intense and sustained efforts that Cavendish must have devoted to the study of electricity. The full range of these researches was only brought to light some 100 years later, when numerous papers and notes left behind by Cavendish were edited and published by J. Clerk Maxwell (Ref. 2, 1879).

Cavendish's aversion to publicity--either for himself or for his work--has done much to create the myth of his life as a recluse or misanthrope. That he was eccentric--even for an outstanding personality of the 18th century--is beyond doubt; but he seems to have been a well-known figure in the scientific society of his day, and a frequent participant in scientific gatherings. His tastes and interests in science were extraordinarily catholic: apart from his experimental and theoretical contributions to electricity he made notable contributions to chemistry, gravitation, heat, magnetism, and meteorology, and he published major papers in these fields, particularly from 1780 onwards. Our main interest here is with his fundamental work on electricity, the nature of which is clearly indicated in the 1771 paper, although the full details and implications of his work were only brought to light in the documents published in 1879. Because there was so much in them that was entirely new and original that was not published at the time, there is a tendency to underestimate the influence of Cavendish's work on the development of electrical science. But, as we shall see, the one or two papers that he did publish were so fertile in ideas and results, that it is hard to believe that those who did study them were not strongly influenced by and indebted to Cavendish's work.

Like everything about him, Cavendish's style in electrical research was strongly individualistic. Newton was clearly his ideal; but the subject and circumstances of his investigations were so different, that, especially in his experimentation, Cavendish soon created a style of his own. He was primarily an experimenter, one with a determined, almost fanatical sense of measurement and quantification. But his aim was not simply to make new or more precise measurements of phenomena that were already discovered and qualitatively well understood. On the contrary, his researches ranged widely among phenomena--in electricity, in chemistry, in geology--where the phenomena were so complex and variegated, and consequently ill-understood, that what was significant for measurement had first to be sought out, clarified, and identified. This could only be done with care, patience, and sustained labor, combined with great experimental skill, powerful insight, and some mastery of mathematical-analytical subtlety! These are characteristics that mark most of Cavendish's work. In addition his personal temperament, his indifference to acclaim by his contemporaries (and possibly by posterity!), his steadfast, unperturbed, virtually single-minded devotion to his researches, (to the exclusion of all other interests), mark his researches with a thoroughness that was quite unusual in his day, for the subjects he investigated.

He worked in an age of style and elegance, but both his personal habits and scientific work are characterized by extreme austerity.

> "The culture of the external senses, which the prosecution of researches in the physical sciences, secures to all who are successful in their study, did nothing in Cavendish's case, to quicken the perception of beauty, whether of form or sound or colour. Many of our natural philosophers have taken great delight in one or other of the fine arts. For none of these does Cavendish seem to have cared.... he was indifferent to elegance of form in his apparatus, which provided it were accurately constructed might be clumsy in shape and of rude materials. He insisted, however, on its perfect accuracy.
>
> Cavendish seemed to have in view in construction, efficiency merely, without attemtion to appearance. Hard woods were never used, excepting when required. Fir wood (common deal) was that most commonly employed. The same disregard for mere appearances was shown in his laboratory."
> (Wilson, Ref.1 p. 178)

Although he made measurements of great precision and delicacy, and often discerned, examined, and established fundamental quantitative relationships where others had only perceived a bewildering array of complex appearances, his apparatus was, as his biographer George Wilson emphasizes, often, at least superficially, of surprising crudity. Cavendish could well be designated as the real originator of the "string and sealing-wax" technique (both were liberally employed by him). It is not unfitting, then, that it was in the Cavendish Laboratory in Cambridge, where some 150 years afterwards in the laboratory endowed by Henry Cavendish's descendent and named in his honor, that this modest and deceptively simple research technique flourished so magnificently in the era of J.J. Thomson and E. Rutherford.

Cavendish's world seemed to be entirely one of dispassionate, intellectual exploration, analysis and measurement. The same biographer writes:

"His theory of the Universe seems to have been that it consisted solely of a multitude of objects which could be weighed, numbered and measured; and the vocation to which he considered himself called was to weigh, number and measure as many as his allotted three-score years and ten would permit....

Throughout his long life he never transgressed the laws under which he seems to have instinctively acted. Whenever we catch sight of him we find him with his measuring-rod and balance, his graduated jar, thermometer, barometer, and table of logarithms, if not in his grasp, at least near at hand. Many of his scientific researches were avowedly quantitative. He weighed the Earth, he analysed the Air, he discovered the compound nature of Water, he noted with numerical precision the obscure actions of the ancient element fire: Each, like some visitor to a strange land, was compelled to submit to a scrutiny, in which not only its general features were noticed, but everything pertaining to it, to which a quantitative value could be attached, was set down in figures, before it went forth to the scientific world with its passport signed and sealed. The half-mythical calendar of the Hindoos... the electricity of the Torpedo, the freezing of mercury, the appearance of an Aurora Borealis, the hardness of London pump water, the properties of carbonic acid and of hydrogen, and much else, were equally subjected to a canon which knew of no limitations, and required that every phenomena and physical force should be held to be governed by law, and admit of expression in mathematical or arithemetical symbols."(Ref. 1, pp. 186-7)

Not all the results of Cavendish's scientific deliberations "went forth into the Scientific World." Especially in his electrical researches, only the major conclusions were published. These were contained in the two papers previously mentioned. Even a glance at these papers suggests Cavendish's extraordinary grasp of the principles involved, and the thoroughness and care--both experimental and theoretical--which he has devoted to his investigations. When the more complete content of his work was brought to light--100 years later by Clerk Maxwell--there was then revealed the full extent to which Cavendish mastered the subject, how he was able, in his solitary way, to develop methods and concepts far beyond the range of his contemporaries, and to anticipate "nearly all the great facts in common electricity which at a later period were made known to the scientific world through the writings of Coulomb and the French Philosophers." (Ref. 1,p. xi)

Before we turn from Cavendish himself to a more detailed discussion of his electrical researches, it is well to recall that these latter comprised only a small part of his life's work in science. This much seems clear from the unpublished papers which had, according to the biographer Wilson, already in 1851, been examined by a leading authority on electricity, Sir William Snow-Harris, who had attached the following note to the lid of the box containing them.

> Of the 20 parcels of papers on electricity 18 belong to the years 1771, 1772, 1773...the remaining parcels are dated 1775, 1776, and are evidently connected with the author's celebrated paper on the Torpedo published in 1776. The papers belonging to the years 1771, 72, 73 consist of six papers on Mathematical Electricity, nine experimental papers, one of Diagrams and figures, and the remainder are of a miscellaneous character.

Of the more than 50 years--from leaving Cambridge in 1753 until his death in 1810--which Cavendish devoted to science, in only five was he preoccupied with Electricity. His publications in chemistry, although also sparse in relation to the efforts he expended, are more extensive. He was one of the prominent pioneers in the development of "pneumatic-chemistry"--the study of chemical processes and properties of gases--and his first published paper (1766), entitled "On Factitious Airs," was in this field. In the history of chemistry he is celebrated for his quantitative studies of the (chemical) properties of hydrogen and carbonic acid (carbon dioxide- "fixed air"), the composition of atmospheric air, and the compounds of nitrogen and oxygen (work published in several major papers between 1766 and 1785); and above all for his pioneering work (1781-1784) in demonstrating, by a quantitative investigation of the electrical "explosion" of a mixture of hydrogen ("inflammable air") and oxygen ("dephlogistriated air"), that water was a compound of these two substances. In this work he became unwittingly embroiled in a controversy on priority and plagiarism with his contemporaries the famous chemists Joseph Priestley and Antoine Lavoisier and the great engineer James Watt. The clamor of this controversy still echoed more than 50 years later, by which time admirers and champions of Cavendish had founded the "Cavendish Society" as a tribute to his contributions to Chemistry(cf.Ref. 1 p.v). It was this society that commissioned the Biography of Cavendish to which we have referred: its compositon and the Biography itself, reflect Cavendish's contributions to chemistry.

Of Cavendish's contributions to physics--other than electricity--by far the best known is his determination, in 1798 (Philosophical Transactions, Vol.17 ), of the weight of the

the earth--or, as it is now referred to, the Universal Gravitation Constant. This was a superb experimental achievement which must have demanded thorough and patient attention to both principle and detail, and scrupulous honesty in observation and measurement. His result was not significantly bettered for nearly 100 years.

From the accounts of his contemporaries, Cavendish died (1810) as he lived: simply, quietly, alone; intellectually and dispassionately fully aware of what was happening, regarding his own death as a natural event in the rational, universal scheme of things.

Scientific Papers of Henry Cavendish Published During His Life-time (cf. Ref. 2)

| | |
|---|---|
| 1766 | "Three papers containing Experiments on Factitious Airs" |
| 1767 | "Experiments on Rathbone Plan Water" (analysis of pump water) |
| 1771 | "An Attempt to Explain Some of the Phenomena of Electricity by Means of a Elastic Fluid." (Phil. Trans. 61) |
| 1772 | "Experiments on Fixed Air, or that Species of Factitious Air which is Produced from Albative Substances, by Solution in Acids or by Calcination." |
| 1776 | "Attempts to Imitate the Effects of the Torpedo."(PT 66) |
| 1776 | "An Account of the Meteorological Instruments Used at the Royal Society's House." |
| 1783 | "Observations on Mr. Hutchin's Experiments for Determining the Degree of Cold at which Quicksilver Freezes" (PT 73) |
| 1783 | "An Account of a New Eudiometer." |
| 1784 | "Experiments on Air." (PT 74) |
| 1786 | "An Account of Experiments made by Mr. John McNab at Hurley House, Hudson's Bay Relating to Freezing Mixture (PT 76) |
| 1785 | "Experiments on Air, second series." |
| 1788 | An Account of Experiments made by Mr. John McNab at Albany Post, Hudson's Bay Relative to the Freezing of Nitrous and Vitriolic Acids." (PT 78) |
| 1788 | "On the Conversion of Mixtures." (PT 78) |
| 1790 | "Height of a LUminous Arch." (Aurora Borealis, 1784) (PT 80) |
| 1792 | "The Civil Year of the Hindoos." |
| 1797 | "A Method for Reducing Lunar Distances." |
| 1798 | "Experiments to Determinate the Density of the Earth."(PT 17 ) |
| 1809 | "On the Division of Astronomical Instruments." |

ALL PAPERS WERE PUBLISHED IN THE PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY (P.T.)

## III Electrical Science in the Mid-18th Century

The history of electricity up to the time of the discovery of the Leyden Jar and the great advances made by Benjamin Franklin and his contemporaries (1747-1755) has been sketched in connection with those pioneer investigations.* Here we shall briefly summarize the conclusions of these works and indicate how these were appraised, extended, and revised in the couple of decades that elapsed before the publication of Cavendish's first paper on electricity.

By the end of the Franklin period, the general picture had emerged of electricity as some sort of "fluid" (imponderable! invisible!) of which ordinary bodies "in equilibrio" had a normal complement, but which by suitable physical agencies (typically rubbing) could be transferred from one body to another. In this way electrified bodies were produced: those with an excess of electric "fluid" were designated "positively electrified," and those with a deficit, "negatively electrified"! In all such processes no electric fluid was created; it was only transferred Electrified bodies represented a disturbed state, one of disequilibrium, which if the opportunity were provided, would return to the normal equilibrium state by transfer of electric fluid from the region of the excess to that of the deficit, i.e. the dissolution of the state of electrification, or the "discharge" of the electrified bodies. For such discharge to occur, a physical medium--a "conductor" of electricity in which the electric fluid could move readily--was required. All metals and some other substances were conductors; many other bodies (insulators) were not, unless the electric disequilibrium was so great that a violent, disruptive discharge occurred (e.g. a spark discharge through air). Bodies easily electrified (by rubbing)—"electrics per se"—were not good conductors; and vice-versa. Different parts of the electric fluid appeared to mutually repel each other; as a consequence, the electric fluid on a "charged" conductor would reside on--or near--the surface.

All observations were consistent with the assumption of a <u>single</u> electric fluid. This was the hypothesis that Franklin adopted, although those who supported the rival theory of <u>two</u> sorts of electric fluid were quick to point out that the experiments, and Franklin's arguments, were not unequivocal in the matter. Possibly the single fluid hypothesis was the simplest and most appealing--especially for the limited range of phenomena studied by Franklin. But the "existence" (already discovered about 1730

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* c.f. History of Physics Laboratory. Notes for the experiments on: "Benjamin Franklin and the Conservation of Electricity." Ref. 3.

by Dufay) of two sorts of electricity, vitreous and resinous (Franklin was unaware of this until after most of his researches had been completed. cf. Ref. 3, p. ), somewhat diminished the appeal of the one-fluid theory, notwithstanding the ready identification of vitreous with "positive" and resinous with "negative" electrification.

The chief difficulty lay with the wholly inadequate understanding of the forces between electrified (positive or negative) objects. The one fluid theory was an excellent working hypothesis in matters to do with the initial and final dispositions of electricity, and its (mechanically unobservable) transfer. But since the one-fluid model identified resinous with a deficit of fluid, it was hard to understand why two bodies, both resinously charged, would repel each other. In fact the generally observed symmetry--as far as forces were concerned--between vitrious (excess) and resinous (deficit) charged bodies was quite a mystery for the one-fluid theory. On the other hand a (symmetrical) two-fluid theory was tailor-made to explain such an experimental fact.

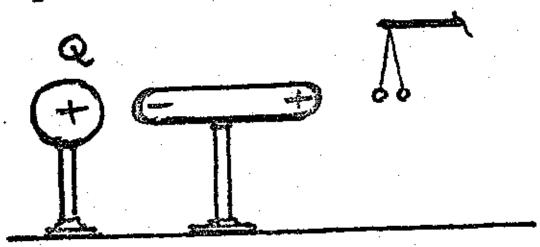
The one-fluid theory could be rescued from this difficulty by postulating that, in addition to the (repulsive) forces between the particles of the electric fluid, and the forces (attractive) between these and the ("fixed") particles of ordinary matter, these latter also exercised, at "large" distances, a mutual repulsion on each other. It need hardly be said that there was some reluctance to countenance such a drastic assumption, one that seemed to contradict the impressively confirmed, well-entrenched, Newtonian Law of universal (gravitational) attraction between all particles of (ordinary) matter. Any proposal that even appeared to challenge the authority of a Newtonian principle was likely, in the mid-18th century, to be received with more than a little scepticism, if not with contemptuous dismissal. The notion--so commonplace today--that there could be a precise symmetry between the electrical forces of repulsion acting between the residual (positively charged) particles of "ordinary" matter and the same repulsion between the (negatively charged particles of the electric fluid would undoubtedly have seemed an extraordinary hypothesis. How could it happen that two such different entities--the "ordinary" matter and the electric "fluid" could exhibit identical forces? Only reluctantly was the concept of forces between the ordinary particles of matter accepted as an inescapable hypothesis, one which it was necessary to accept in order to interpret the observed phenomena.

Apart from this manifest dilemma, there was also a general obscurity, or at best a very incomplete uncerstanding of the quantitative features of these "electrical" forces--the manner in which their influence extended into space. For some time, this confusion was compounded by lack of a clear distinction between the spread of the electric fluid itself and the range over which its forces were exhibited (e.g. the concept of "electric atmosphere" so popular with Franklin). There was still apparently, in some quarters, a reluctance to accept the (Newtonian) concept of action at a distance for the electric particles; or perhaps a feeling that this abstraction might be avoided in the explanation of electricity. After all, Newton and gravitation notwithstanding, a "real" explanation of forces in terms of contiguous interaction has always retained its appeal; (and indeed--in a far more sophisticated and abstract form--is the assumed principle for all interactions today! "Action at a distance" is a feature of pre-Einsteinian physics!)

In the period between 1750 and 1770 a variety of experiments by Benjamin Wilson, Robert Symmers and John Canton (in London), Ebenezer Kinnersley (in Philadelphia), Franz Ulrich Aepinus (in Berlin), and Johann Wilke (in Stockholm) had done much to clarify the general nature of electric forces, and to connect some of the obscurities and misconceptions about these which were associated with Franklin's ideas. Wilson (1759/60) experimentally demonstrated that the "impermeability" of glass to the transmission of electricity--which played such a central role in Franklin's theories--was not always true. This was simply shown by rubbing (and so charging) one side of a sheet of glass, and drawing off charge from the other. Whether or not this "leakage" of charge through glass (which is of course a relatively <u>slow</u> phenomenon) is significant--in a quantitative sense--in the interpretation of the Leyden Jar action, Wilson does not consider; but his observations and criticism do stimulate more quantitative enquiries about the electrical properties of materials, in contrast with the earlier tendency toward explanation in terms of "absolute", ideal, qualitative features. Thus Aepinus--in a letter to Wilson (1761)--correctly interprets Franklin's concept of impermeability in a relative sense: glass <u>is</u> impermeable when compared with the "ease" with which it moves in the wire (or the spark in air) which forms part of the discharging circuit of the Leyden Jar. Symmers (1760) seems to have been the first actually to <u>measure</u>, mechanically, large electrical forces, although the

the circumstances of his experiments are rather bizarre. He had happened (whilst in mourning), to be wearing two pairs of stockings--white silk ones covered by black worsted,--and had observed that when he removed his stockings, and separated them (in front of a fire), both crackled and sparked, i.e. they were electrically charged. Moreover their charges were "opposite"--under some circumstances considerable force was required to separate them. Symmers investigated the circumstance of this electrification--and measured these forces. He found after a time that he could dispense with his leg--or even with his arm which he had found to be equally effective--as a piece of apparatus. In due course he was led to study the "electrical cohesion" between glass plates whose outer surfaces are covered with charged metal-foils. All this is of topical interest in connection with the electrical forces inside, or at the surface of, an electrically charged body; but Symmer's concepts are rather naive, and the time is not yet opportune for any serious interpretation of such subtleties.

More directly related to the question of the law of force between particles of electrical fluid, and electrified objects themselves (and also contributing very materially to the technique of electrical measurement) are Canton's experiments (1753)--repeated, extended, and interpreted later by Wilke and Aepinus--on electrification without actual contact (i.e., in later terminology, electrification by "induction"). The essential features here are that the presence if a charged object Q near an extended (insulated) conductor, results in a charge of the opposite sign at the proximate points and of the same sign at the distant ones; and that this "separation" of charges persists only as long as Q remains nearby. Canton's interpretation was in terms of the ambiguous "electrical atmospheres," but Aepinus (1759) clearly perceived that the phenomena could be more clearly explained in terms of electrical forces acting between Q and the (mobile) electric fluid in the conductor. This also provided a natural interpretation of Wilke's observation that if the conductor were momentarily "touched" (i.e. electrically "grounded") at any point, Q remaining in position, then the conductor became permanently charged. Incidentally these observations--like all those made before--and most of those made for 100 years or so afterwards!--could be interpreted in the language of either the one fluid or the two fluid theories, provided appropriate assumptions are made about the forces between the particles of electric fluid, those of the "ordinary matter," and between

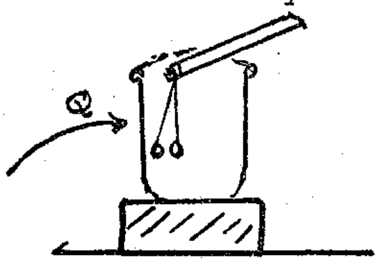


particles of one sort and the other. Aepinus did indeed sketch out such a theory (Ref. 6) which is essentially the same as that which Cavendish later more fully develops (p. 21 below).

Aepinus and Wilke, working together in Berlin, also demonstrated that an arrangement of two flat parallel metal surfaces (one insulated and one "grounded") separated by a small air gap could act as a Leyden Jar, without the use of glass. The particular properties of glass (or any similar substance) were thus eliminated from the interpretation, and instead the action could be explained in terms of electrical forces acting at a distance. The concept of "electrical atmospheres" surrounding charged objects is here again disposed of as irrelevant (Ref. 4a, p. 286 et seq.)

Wilke, applying these basic ideas of his mentor (Aepinus) showed how to interpret numerous further experiments in electricity--including a method for generating indefinite quantities of electricity by induction (the forerunner of many such 'induction machines' of the late 18th and 19th centuries.) He also appears to have anticipated some features of the action of "dielectric" (i.e. an insulating material in an electric field) proposed long afterward by Michael Faraday (Ref. 3, p.52).

Whilst this work of Aepinus et al.firmly established the superiority of the clear principle of "action at a distance" to the obscure notion of "electric atmospheres," it lacked any assumption about the dependence of the electric force on distance, and consequently any of the predictions that might result from such an assumption. The first explicit suggestion that the law-of-force between particles of electricity (and, in the Aepinus theory, also therefore between particles of ordinary matter) was apparently made by the famous "chemist" Joseph Priestley in 1767 (Ref. 4b pp.372-376 ). He repeats an experiment of which Benjamin Franklin has sent him an account seeking Priestley's advice. The experiment is a simple one: cork or pith balls (a la Canton) suspended inside an electrified tin cup, appear in no way to be influenced by the electricity. Neither do they acquire any electricity if suspended in the cup for a long time, and removed after the cup is discharged; and only a very minute charge if they have at some time touched the inner surface of the cup.




Priestley boldly and brilliantly draws the correct conclusion:

> "May we not infer from this experiment, that the attraction of electricity is subject to the same laws as that of gravitation, and it is therefore according to the squares of the distances; since it is easily demonstrated, that were the earth in the form of a shell, a body in the middle of it would not be attracted to one side more than another."

The demonstration of this proposition was indeed one of the important achievements of Newton. (Principia, Proposition LXX, Theorem XXX).

It hardly escaped the notice of many investigators (Aepinus especially) that there were many analogies between electric and magnetic interactions--but there are important differences. Moreover, the suggestion--with experimental confirmation--that the law of inverse squares applied to magnetic forces had already been proposed by John Michell in 1750 (Ref. 5) and had been confirmed or affirmed by several other investigators in the subsequent decade or two (Ref. 3, pp. 53, 56-67). With Newton's example and authority before them, and with Michell's and Priestley's demonstrations for magnetism and electricity respectively, it was not difficult to understand why the law of inverse squares was the "natural" one to adopt--once a general physical scheme for action at a distance had been arrived at. Nevertheless, a clear, quantitative, unequivocal, experimental demonstration that this was the law of electricity, and an appreciation--and formal demonstration--of the implications of this law for the distribution of electricity on conducting bodies had yet to be made. This was the achievement of Henry Cavendish.

## IV. The Cavendish-Aepinus Theory

(Cavendish: "An Attempt to Explain Some of the Phenomena of of Electricity by Means of an Elastic Fluid" Phil. Trans. 1771.)*

(Aepinus: "Testamen Theoriae Electricitatis et Magnetismi," 1759.) +

The full account of Cavendish's experimental demonstration of the inverse-square-law for electricity, unlike many of his other electrical researches, was not published until a century after it was performed.

But his fundamental analysis of electrical phenomena in the two major and lengthy papers he did publish (in 1771 and 1776) contains not only a fully developed theory, based on a few clearly formulated principles and assumptions; but also gives

*J.C. Maxwell's commentary on Cavendish's theory is appended (Appendix p. 50).

+A good (English) account of Aepinus's theory is goven in John Robinson's (1739-1805) 'System of 'Mechanical Philosophy' (1804/1822). This is based on his Encyclopedia Brittanica articles on Electricity, Magnetism etc. (1803).

a clear indication that much of his theory had been confirmed by experiments, that he had already (in 1771) performed. It is perhaps the extraordinary wealth of experimental information and ideas which were subsequently disclosed in his unpublished papers, that have helped create the impression that Cavendish discovered so much but disclosed so little. But there is little doubt that many who followed him owed much--and often more than was explicitly acknowledged--to Cavendish's work (cf. p. 33 for example).

Cavendish's electrical experiments--and his demonstration of the inverse-square-law in particular, are not isolated or disconnected experiments. They all relate to a general theory of the distribution of electricity, and the forces between electrified bodies(p.32). The theory is couched in terms ofthe ideas and terminology of the Newtonian Mechanics of his day, but it is remarkable how, with the proper translation of a few terms into their modern counterparts, the whole logical structure which he creates and exploits, leads to perfectly intelligible and quite valid conclusions. The most "archaic" feature of this theory is its terminology.

Some of the basic features of Cavendish's theory coincide, as already remarked, with those of Aepinus' theory; but as Cavendish justly remarks, his own development takes the subject much further, especially in respect of quantitative arguments. We shall summarize the theory, then, in the form in which Cavendish presents it.

As a basis, he adopts Franklin's one-fluid model. This does not, however, imply that agreement between his theory and experiment is necessarily decisive as between one and two fluid theories. Apart from the basic postulates of the theory, there are additional, arbitrary assumptions--for example the manner in which electricity is constrained from leaving a charged conductor, or the maximum density of fluid which must be present in a conductor--which have to be invoked; and these may be much easier to envisage, or less arbitrarily justified, in the one fluid than in the two fluid model. But in either model some arbitrariness is invoked.

The basic principles are lucidly and succinctly stated by Cavendish at the outset:

" 3) There is a substance, which I call the electric fluid, the particles of which repel each other and attract the particles of all other matter with a force inversely as some less power of the distance than the cube: the particles of all other matter also, repel each other, and

and attract those of the electric fluid, with a force varying according to the same power of the distances. Or, to express it more concisely, if you look upon the electric fluid as matter of a contrary kind to other matter, the particles of all matter, both those of the electric fluid and of other matter, repel particles of the same kind, and attract those of a contrary kind, with a force inversely as some less power of the distance than the cube.

4) For the future, I would be understood never to comprehend the electric fluid under the word matter, but only some other sort of matter.

5) It is indifferent whether you suppose all sorts of matter to be indued in an equal degree with the foregoing attraction and repulsion, or whether you suppose some sorts to be indued with it in a greater degree than others; but it is likely that the electric fluid is indued with this property in a much greater degree than other matter; for in all probability the weight of the electric fluid in any body bears but a very small proportion to the weight of the matter; but yet the force with which the electric fluid therein attracts any particle of matter must be equal to the force with which the matter therein repels that particle; otherwise the body would appear electrical, as will be shewn hereafter.

To explain this hypothesis more fully, suppose that 1 grain of electric fluid attracts a particle of matter at a given distance, with as much force as n grains of any matter, lead for instance, repel it: then will 1 grain of electric fluid repel a particle of electric fluid with as much force as n grains of lead attract it; and 1 grain of electric fluid will repel 1 grain of electric fluid with as much force as n grains of lead repel n grains of lead.

6) All bodies in their natural state with regard to electricity, contain such a quantity of electric fluid interspersed between their particles, that the attraction of the electric fluid in any small part of the body on a given particle of matter shall be equal to the repulsion of the matter in the same small part on the same particle. A body in this state I call saturated with electric fluid: if the body contains more than this quantity of electric fluid, I call if overcharged: if less, I call it undercharged. This is the hypothesis; I now proceed to examine the consequences which will flow from it."

(ref.2 p.3/4)

The significance of the term "elastic fluid" used in the title is illuminated by some of Cavendish's unpublished papers, written before, entitled "Thoughts Concerning Electricity" (Ref.2 pp. 94-103). From these it appears that Cavendish may have originally conceived the electric fluid theory through the mutual repulsion of its parts as exerting "mechanical" pressure on contiguous parts of the fluid, in analogy with the "hydrostatic" forces in familiar "mechanical" fluids. In this situation, such forces act "locally"--i.e. across the boundary between one fluid element and another. (Long-range "body" forces--such as gravity--may also act, typically from outside ). In the actual development of the theory such "hydrostatic" forces are not invoked. The electrical forces are those acting between any one particle and all other particles at whatever distance; although distant particles have diminishing influence in accord with some law of reduction of force with distance. In this sense, Cavendish's is a correct theory of electrostatics.* The sense in which Cavendish's "electric fluid" is "elastic" is that more (or less) than the normal amount (i.e. the amount which an uncharged body possesses--what Cavendish terms the "saturated" amount) can be "compressed" into a given body. There is also an implied limit to the maximum density of the fluid but this concerns some secondary assumptions of the theory which do not necessarily affect its validity. Cavendish seems to have changed the essential basis of his theory, although he retained (forgetfully?) the term "elastic" in the title.**

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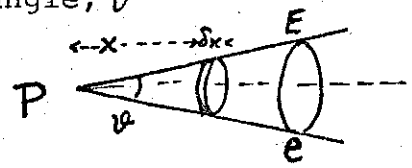
* In contrast, more than 50 years after Cavendish, and possibly misled by the term "fluid," Ohm treats the flow of electricity (galvanic current) in terms of such a "local-pressure" model.

**It is important to recognize that even in the "classical" theory of electrostatics as expounded today, there are some (often partly hidden) assumptions which do not form part of the theory proper. For example, what forces prevent the electric charge from leaving a charged conductor in vacuo? What is the thickness of the layer of surface charge on a conductor? Is there a maximum amount of displaceable charge in a conductor? We recognize today that the answer to these questions includes detailed atomic, quantum-mechanical considerations, far outside the domain of classical electricity. But recognition of the distinction of what must be the

A most important theorem which Cavendish proves at the outset (Ref. 2. p.5) is intimately related to this notion of "fluid pressure." He considers any point in a region of uniform (or approximately so, over a small region) charge; and shows that if the law of force is inverse $r^n$, then for $n<3$, the effect of the forces due to the (infinitesimal) local region are unimportant, in comparison with the forces due to the bulk of the electricity at finite distances, whereas for $n>3$, the reverse is true (i.e. the force would be local). Thus the assumption that the force is "inversely as some less power of the distance than the cube"--in § 3, quoted above,--is essential to the theory of what one might call "long range" electrostatic forces, in contrast to a short range "hydrostatic pressure" type theory.

Since this is so fundamental to the whole theory, we give the proof in Cavenidsh's terms, only substituting contemporary terms for the archaic, Newtonian, terms: fluxion, fluent, etc.

P is a point in the region of charge density $\rho$ which, locally, does not vary abruptly. PEe is a cone of small half-angle, $\vartheta$




The charge in frustrum at distance x from P is: $\pi\rho(x\vartheta)^2\delta x$
The force due to this on a "particle" at P will then be proportional to $\pi\vartheta^2\rho x^2\delta x/x^n$. The total force on all the charge in the cone out to a distance a will be:

$$\int_0^a \pi\vartheta^2\rho x \quad dx = \pi\vartheta^2\rho a^{3-n}/(n-3)$$

Clearly, if $n<3$, this force vanishes as a becomes small; i.e. "local" charges have vanishingly small effect. Conversely, if $n>3$, only the "local" effects are significant. Formally, in this case they become infinite! In actuality one would need--in such an eventuality--to understand the granular structure of the charge distribution before any physical significance could be given to this type of cal-

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subject of detailed physical investigations, and what could be created on the basis of formal (idealized) assumptions, was not easily made in Cavendish's day. Indeed a clear recognition of the distinct nature of these different types of problems has only slowly emerged in the subsequent two centuries.

culation.*

From his basic assumptions Cavendish shows that there is an overall symmetry between "overcharged" and "undercharged" bodies and that for the consideration of forces between bodies, one only need consider the actual surplus or deficit of charge. All this is consistent with the experimental observations of the forces between like and unlike charged bodies.

Of immediate significance for the law of force between charges is the demonstration (c.f. Newton's theorem for inverse-square-law forces, p. 20 above)--that from the inverse-square-law it follows that in equilibrium, all the excess ( or deficiency) of charge on an electrified spherical conductor will lie on a surface layer. Inside this layer, i.e. throughout the bulk of the material, the conductor will be uncharged (in Cavendish's terminology, "saturated" with electric fluid). Any law other than the inverse-square could not give the necessary equilibrium conditions for the ("saturated") fluid inside.** For $n > 2$, Cavendish asserts that a charge inside would "be impelled towards the centre"; conversely for $n < 2$, "it would be impelled from the centre." (This result is a generalization of the proposition that for a hollow, spherical, charged conductor, there is no electrical "field" inside. It was from a general experimental observation of this sort--that there is no electrical force inside a closed hollow conductor of any shape, that Priestley conjectured the inverse-square-law--Cf. p. 20 above.)

It should be noted that there is a great difference between the elementary proof, as found in any textbook, that a uniform spherical layer of charge produced no electric force inside the sphere--if the force law is inverse square; and the more general problem of what would be the actual distribution of charge in a solid (spherical) object if the force law is <u>not</u> inverse-square.*** It is obvious that in

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*Such situations do exist in physics! Forces (ultimately electrical) between atoms can be expressed--approximately--as of the $r^n$ type, and with $n > 3$. This corresponds to the "short range" forces which are responsible for the cohesion of solids, etc. These are not considered in the domain of "classical electrostatics".

**Or, in current terminology, would not create a zero electric field inside the conductor; which is necessary, since only then could there be electrostatic equilibrium.

***Cf. Appendix p. 50

this latter case, an equilibrium, uniform surface distribution of charge is consistent with an inverse-square-law; but is this the only law that is so consistent? Cavendish attempts to calculate the actual distribution for any n between 2 and 3. Apart from the analytical problems such a problem poses, he becomes involved, unavoidably, with the specific (unverifiable!) features of the theoretical model: Just how compressed can a "surface layer" be? What are the other "forces"; those which prevent the charge from leaving the surface; and those which determine the ultimate maximum (or minimum) possible density of charge under any circumstances? As for the latter, he postulates that there is a maximum amount of "fluid" which may be compressed into any body, that is to say, at some very short (atomic?) distance the particles of electric fluid must begin to repel each other more strongly than the inverse-square-law implies! In the case of an undercharged body, he assumes a finite limit to the amount of fluid in body and therefore a finite limit to the depletion. Both assumptions lead to the same formal result when the law is inverse-square: In either overcharged or undercharged conductors, the electrified (i.e. the "non-saturated") region is a thin layer at or very near the surface--ideally the surface itself--whose exact thickness (not measureable experimentally) depends on considerations other than the formal assumptions of the theory and the law of inverse squares. (This same feature exists in classical electrostatic theory today.)

For $n \neq 2$, different models yield different results. A complete treatment beyond the range of mid-18th century mathematics. The first formal analysis of this problem was made by Green (of the celebrated Green's function) in 1833 , using a symmetrical two fluid theory. Maxwell (Ref. 2 p. 368)*, in his presentation of this analysis, comments that Cavendish's speculative conclusions are qualitatively correct.

To ensure that no charge leaves the surface--a condition that is applicable to all charged conductors in insulating surroundings--Cavendish invokes the "impenetrability" of the surrounding atmosphere to the electric fluid; or more generally:

> " I shall always suppose the bodies I speak of to consist of solid matter, confined to the same spot, so as not to be able to alter its shape or situation by the attraction or repulsion of other bodies on it: I shall also suppose the electric fluid in these bodies to be moveable, but unable to escape, unless when otherwise expressed. As for the matter in all the rest of the universe, I shall suppose it to be saturated with immoveable fluid." (p. 16/17 Ref. 2)

* Appendix p. 50

(Cavendish implies, earlier in this context, that if the fluid in the surrounding air is capable of some (limited?) movement, the sphere will be surrounded with an infinitesimally thin layer of electricity of the opposite sort. This notion--had it been elaborated--might have led to the concept of "polarisation" of charge rather than free charge. But the general concept of "displacement" and "polarization" of charge is nowhere clearly developed in this work. It is not introduced until 70 years later, by Faraday.)

In general—probably, as Maxwell comments, because electrified bodies lost charge due to poor insulation—a much greater role was attributed to the air in the theories of this period than was properly justified. As techniques improved, this, usually extraneous factor, was gradually eliminated.

. . . . .

In the further development of this theory, Cavendish examines the distribution of electricity in a variety of other arrangements, which appear less immediately related to the question of the law-of-force, but are, in fact, extremely important in this context, since many of the results Cavendish arrives at, although formulated for any value of n between 2 and 3, are only consistent with experiment --even qualitatively--if the law is actually inverse-square. For example, for parallel plates with different charges, he concludes:

> From the... foregoing problems it seems likely, that if the electric attraction or repulsion is inversely as the square of the distance, almost all the redundant fluid in the body will be lodged close to the surface, and there pressed close together, and the rest of the body will be saturated. If the repulsion is inversely as some power of the distance between the square and the cube, it is likely that all parts of the body will be overcharged: and if it is inversely as some less power than the square, it is likely that all parts of the body, except those near the surface, will be undercharged. (p. 17)

. . . . . . . . .

A most important concept, in relating a theory of the distribution of electricity to actual observations, is that of a connecting wire made of material which is electrically conducting. The principle by which the correspondence is to be made here between theory and experiment is often dismissed quite cavalierly: apart from specifying that the wire be there, the particular form it takes is assumed unimportant--it simply "connects" (electrically) two (or more) electrified bodies. Itself, it carries essentially no charge. It is not difficult to justify these assumptions in the usual theory of electrostatics. One simply argues that such an "ideal" wire maintains the connected conductors at the same "potential." But in the early development of the subject, when concepts we now take for granted were in the process of being formed, and when it was by no means easy to discover just what approximations were physically useful or justifiable, many of the questions whose answers we now take for granted--or even dismiss without consideration at all--needed to be, and were in fact, more carefully scrutinized.

Cavendish defines a "canal" as follows:

> By a canal, I mean a slender thread of matter, of such kind that the electric fluid shall be able to move readily along it, but shall not be able to escape from it, except at the ends, where it communicates with other bodies. Thus, when I say that two bodies communicate with each other by a canal, I mean that the fluid shall be able to pass readily from one body to the other by that canal. (Ref. 2, p. 17)

To understand the essential features and function of such a connecting "canal" it is no surprise to find that Cavendish has to establish a concept equivalent to that of "potential": in his words, "degree of electrification." It is in this context that Cavendish defines the terms "positively" and "negatively" electrified.

> In order to judge whether any body, as A, is positively or negatively electrified: suppose another body B, of a given shape and size, to be placed at an infinite distance from it, and from any other over or undercharged body; and let B contain the same quantity of electric fluid as if it communicated with A by a canal of incompressible fluid: then, if B is overcharged, I call A positively electrified: and if it is undercharged I call A negatively electrified; and the greater the degree in which B is over or undercharged, the greater the degree in which A is positively or negatively electrified. (Ref. 2, p. 45)

This definition is completely equivalent to that of the sign of the "potential" in current terminology. The potential of an object at "infinite distance" is positive or negative according as its total charge (algebraically) is greater or less than zero (i.e. "over" or "under-charged").

The "canal of incompressible fluid" is equivalent to an ideal wire which ensures that the two bodies are at the same potential. The significance of "incompressibility" may not be at once obvious: it is not so much a question of the forces exerted on or by the fluid, as a statement that there is neither less nor more than the saturation amount of "fluid" in the "canal,"* i.e. the connecting wire carries no significant charge.

Equipped with these concepts: conductor and insulator, 'over-charged' and 'undercharged', 'positively and negatively electrified', 'connecting canal', etc. Cavendish is able to explain--at least qualitatively--all the typical conditions for electrification by induction. At times the analogy, taken literally, suggests problems which are not manifest in "classical" electrostatics as we now understand it. For example, what if a conductor, strongly over-charged, "drives out" all the electric fluid from a nearby wire? This situation might be comparable to a syphon for which "the height of the bend of the syphon above water in the vessel is greater than that to which water will rise in vacuo." Today, we would regard such extreme limiting conditions as far beyond the realm of conventional electrostatics; and indeed Cavendish also implies that he is normally considering only small departures from the equilibrium ("saturation") distribution of electricity. This is, of course, the region where electrostatics--and the usual concepts of conductors and insulators--do, in fact, apply.

The analysis is not limited to qualitative considerations: in some simple geometrical configurations, precise quantitative

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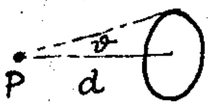
* It is worth commenting again that the residue of terms such as "elastic fluid," "canal" suggest a picture of mechanical motion in the "hydrostatic" type pressures. This is probably the way Cavendish originally conceived his theory: as previously mentioned, parts of his unpublished "Thoughts on Electricity" suggest this. But having started to develop such a theory he soon shows that such a theory, which implied local forces between contiguous matter, required shorter ranged forces, $n > 3$, than seemed likely. On the other hand $n < 2$ leads to all sorts of contradictions. Thus only for $2 \leq n < 3$ does a realistic action-at-a-distance theory seem possible. This is the range of n that Cavendish considers throughout. It clearly excludes the possibility of a hydrostatic fluid-pressure model; although Cavendish seems to have left behind in his terminology occasional traces of the concepts with which he began his investigations, but which these same investigations forced him to discard.

results are given. (Newtonian fluxions are used in these analyses). Thus for a uniformly charged disc, the force on the charged particle on the axis (at P) is shown to be proportional to:

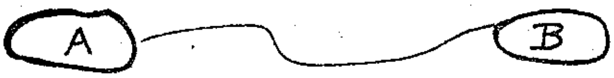
$$d^{(2-n)}\left[1-(\cos\vartheta)^{n-1}\right]$$



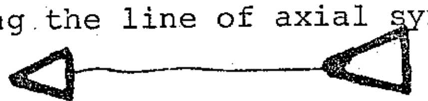

(the force law is $1/r^n$; with $2 \leq n < 3$). For points close to the disc (i.e. $\vartheta \sim \pi/2$), it is pointed out that the force is the same as would be produced by an infinite disc.

For n=2, the force near the surface is proportional to ($1-\cos\vartheta$); and is, therefore, near the disc, a direct measure of the charge surface-density. (The important theorem was "rediscovered" and exploited by later investigators.) The analysis is extended to the consideration of the forces on a column of charge, and to a charge distributed directly over the perimeter of the disc--both useful idealizations of practica. cases. It is also shown that for geometrically similar conductors, the equilibrium (i.e. static) distribution of charge is likewise similar--assuming in both cases the surplus charge (or deficit) resides on a surface layer of negligible thickness. (The only proviso made generally, is that the surplus or deficit of electric fluid is small compared to the total--"saturation"--quantity.)

Another important theorem is proved, one perhaps self-evident today, but certainly not to Cavendish's contemporaries, or even to those writing on electricity in the succeeding couple of decades! Any two (conducting) bodies A,B are connected by an ideal "canal" of any shape: the distribution of charge between A,B (whether a total excess or deficiency) is

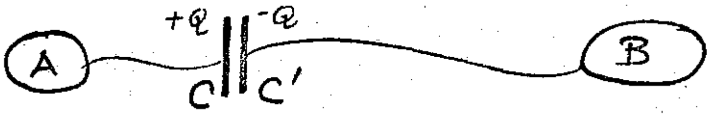



independent of the material of A,B. Consideration of the equilibrium of the "fluid" in the connecting wire (all points are at the same "potential") makes it possible to consider the ratio of the charges of two effectively infinitely separated bodies connected by a long ideal wire, and therefore at the same potential. This then, in later terminology, is the ratio of the capacities of the two bodies. Only simple, symmetrical systems could be treated analytically. Cavendish considers a system of axial symmetry: two similar bodies b,B, well separated and joined by a wire along the line of axial symmetry.

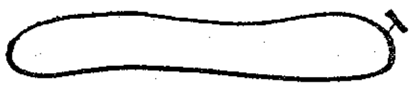


The overall excess (or deficit) of fluid is shared by the two bodies in the ratio $(B/b)^{n-1}$, where B,b represent similar dimensions and the law of force is $1/r^n$; $1 \leq n < 3$. For n=2 we have the well-known result: Capacity ( ≡ charge/potential) is proportional to linear dimension.*

As a corollary, it is shown that the capacity of a flat plate (thichness≪size) is independent of thickness; that for two plates close together connected to other bodies, equal and opposite and uniformly distributed charges will reside on CC',




and small charges on A,B. This is the essence of the "condensor." It is also shown that for a small plate connected to the surface of a large conductor thus:



the charge on the plate is independent of its thickness; a result which is essentially the basis of Couloumb's later 'invention' of the "proof-plane."

Cavendish is well aware of the dependence of his "theorems" on the assumption of ideal "canals" with "incompressible fluids" (i.e. carrying negligible charge). He shows that if these are "infinitely" long and used to connect conducting bodies which are essentially "separate," the distribution of charge between the bodies is the same "whether the canal, by which they communicate, is straight or crooked, or into whatever part of the bodies the canal is inserted."

This is clearly a result of immense practical importance, but to what extent does it depend on the assumption of an ideal canal? "I have good reason however to think," Cavendish adds, "that the propositions actually hold good very nearly when bodies are joined by real canals; and that whether the canals are straight or crooked or in whatever direction the bodies are situated with respect to each other; though I am by no means able to prove that they do...some more skillful mathematician

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*Cavendish refers in his writings to "inches of electricity"--a clear recognition that electrical capacity is of the dimensions of a length. For a fixed degree of electrification (voltage--as determined by a standard spark-gap), this "length" then measures the quantity of electricity (Q=CV).

may be able to show whether they really hold good or not. What principally makes me think that this is the case, is that as far as I can judge from some experiments I have made the quantity of fluid in different bodies agrees very well with these propositions on a supposition that the electric repulsion is inversely as the square of the distance"(Ref. 2 p. 42/43).

Here is an unequivocal statement not only that the law of inverse squares has been confirmed experimentally, and a clear indication (by studying the distribution of charges in equilibrio) of how it has been done, but also an affirmation that the basic approximation used in the course of these tests has itself been subject to test: "It should also seem from these experiments, that the quantity of redundant or deficient fluid in two bodies bore very nearly the same proportion to each other, whatever is the shape of the canal..."(Ref. 2 p.43).

We now know that Cavendish's unpublished papers--later edited by Clerk Maxwell--just how detailed and thorough were these experiments. But even without these details, Cavendish has clearly demonstrated the validity of the inverse-square-law; and perhaps even more significant, how in principle it can be demonstrated with extreme precision. As we shall see later, the principle of Cavendish's method provides--still today--the most sensitive test of this law.

In the second, less formal, part of this long paper, subtitled "Containing a Comparison of the Foregoing Theory with Experiment", Cavendish interprets many of the earlier experiments of Canton, Kinnersley, Franklin and others on electric induction, the Leyden Jar, the effects of points, etc. (Ref. 2 p. 53). In many cases the interpretation rests only on the qualitative features of his one-fluid theory, and the tacit assumption of the existence of bodies that are conductors or insulators. ("What this difference in bodies is owing to I do not pretend to explain.") Although the explicit assumption of an inverse-square-law is not required--nor used--in most cases, little doubt is left that it is this law that is assumed and that conforms best with experiments. For example, in explaining the discharging action of sharp points, Cavendish writes:

> "...if two similar bodies of different sizes are placed at a very great distance from each other, and connected by a slender canal, and overcharged, the force with which a particle of fluid placed close to corresponding parts of their surface is repelled from them, is inversely as

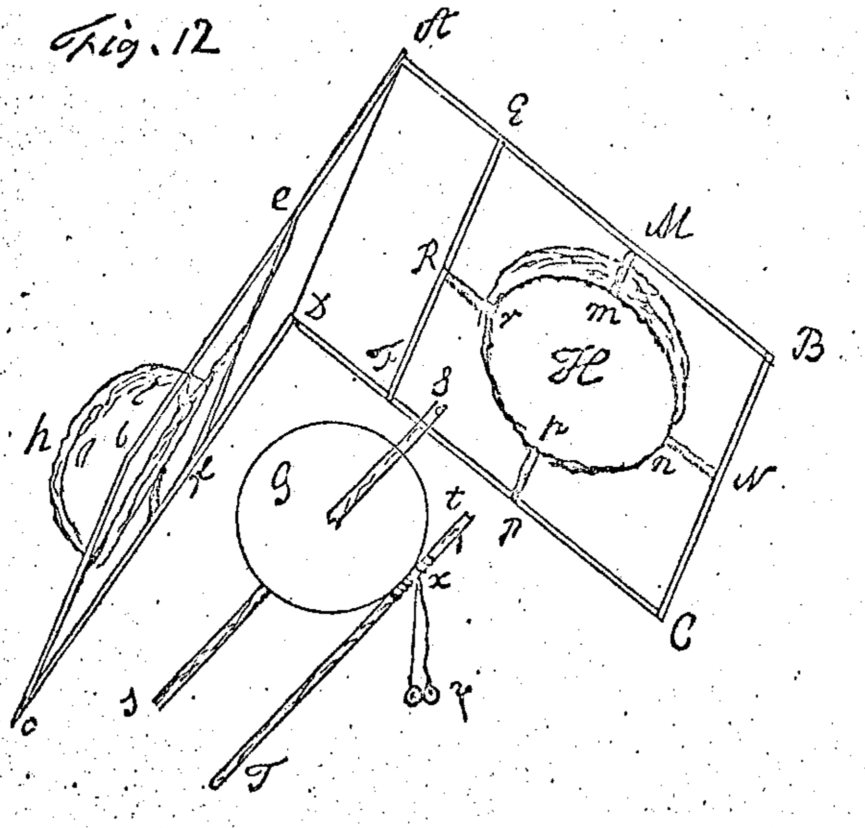
the corresponding diameters of the bodies. If the distance of the two bodies is small, there is not so much difference in the force with which the particle is repelled by the two bodies; but still, if the diameters of the two bodies are very different, the particle will be repelled with much more force from the smaller body than from the larger. It is true indeed that a particle placed at a certain distance from the smaller body, will be repelled with less force than if it be placed at the same distance from the greater body; but this distance is, I believe, in most cases pretty considerable; if the bodies are spherical, and the repulsion inversely as the square of the distance, a particle placed at any distance from the surface of the smaller body less than a mean proportional between the radii of the two bodies, will be repelled from it with more force than if it be placed at the same distance from the larger body." (Ref. 2 p. 53).

. . . . . . . . .

The only other paper on Electricity published by Cavendish--that on the "Torpedo" in 1775/6--has little direct bearing on the law-of-force for electricity. But it can hardly have failed to impress those of Cavendish's contemporaries who read it, with Cavendish's great mastery of both principle and experimental method. Had there been any doubt about the authority of the conclusions presented in his earlier paper, this later work must have lergely dispelled them.

The full impact of some of the other new ideas elaborated in this second paper: the concept and measurement of specific resistance; the relationship between electrical discharge rate (i.e. current), electric force (potential difference) and resistance; the distinction between quantity of charge--producing a shock--and the electrical force(tension, or potential difference) involved --were only fully realized much later in fact not until the discovery of voltaic electricity, c.1800, brought into full relief the contrast between a large flow of electricity with low potential differences, and relatively small quantities with high potentials.

We may summarize Cavendish's contributions, and their influence, by asserting that anyone who perceptively read his published papers, in the 1770's, would have been left in no doubt as to the validity of the interaction at-a-distance of electricity, the virtual certainty of the inverse-square-law; and would have found the general principles and many techniques of electrical measurement and investigation a fertile source of inspiration for further experiments.* It is hard to conceive that his work was not, in fact, so exploited.




Sketch of Apparatus for

Experiment to ascertain the Law of Force

(Ref. 2, p. 104)

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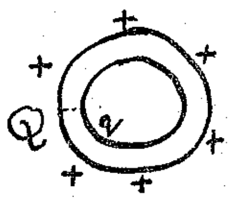
*We might also mention that already in 1772, his authority on matters electrical, no doubt established by his paper by 1771, brought him to be nominated a member of a Committee of the Royal Society appointed to investigate protection (of the Navy's powder magazine!) from lightning strikes. This committee included W. Watson, E. Wilson, and Benjamin Franklin. In a later (1777) report, Cavendish supports the Franklin view that sharp lightning conductors are preferable to blunt ones.

## V. Proof of the Inverse Square Law

Cavendish's own description (unpublished until 1879!) of his experimental proof of the inverse-square-law is of such exemplary clarity that it needs little explanation or supplementation. Clerk Maxwell describes it as "one of the most perfect examples of scientific exposition; truly worthy of the experimental classic it describes..."

The apparatus we have constructed is in dimensions and principle of operation identical with Cavendish's' and where practicable we have used the same simple contraptions of silk, string, glass and sealing wax. However, we cannot expect to match the effort and time that Cavendish devoted to his researches or the spacious quarters of his laboratory, even if one could match his skill. Moreover, there is an atmospheric problem! Electrostatic experiments (this is not true of the usual electro-magnetic ones..Why?) are notoriously dependent on the humidity. On clear, dry, frosty days conditions are excellent: but on the particular day you happen to attempt the experiment, conditions may be otherwise. The problem, of, course is due to the imperceptible layer of water--a good conductor--that may condense on the surface of the (glass) insulators and temporarily destroy their insulating properties. In critical places we have then substituted modern insulating materials--less susceptible to the climatic vagaries--for glass. (See Experimental Notes.) Otherwise the technique, like the principle, is Cavendish's.

The principle is simple: two concentric hollow spheres, with conducting surfaces, are connected by a thin wire. A charge (Q), say, "positive", is deposited on the outside. If the inverse-square-law is true it will stay there: no charge




(q) will move to the inside sphere. his can be tested. For the experiment to be a <u>sensitive</u> test of the law, <u>that is for small</u> departures from n=2 to <u>be detectable</u>, there are three criteria to be satisfied!

i) the geometrical arrangement should be one in which a relatively large q results from the small departure from the inverse-square law;
ii) the instrument used to examine the charge q--if any-should be as sensitive as possible; provided that
iii) extraneous charges are not allowed to influence this instrument; and no charge can reach the inner sphere when it is <u>not</u> enclsoed by the outer one--for example by charge leaking slowly down the (glass) support of the inner sphere (of G in fig. p. 31 above).

Cavendish typically deals with these problems by perceptive design and experiment rather than by the development of new refined instruments. In fact, he used the accepted rather crude techniques of his day--but with a great deal of intelligence.

The outer sphere is split into two hemispheres, and each is supported on glass stems; the inner sphere is mounted axially on an insulating glass rod. A commonplace friction-machine is used to charge the outer sphere: to detect the charge in the inside (after the outer-spheres have been opened) nothing more elaborate (or imaginative) than a pair of suspended pith-balls, as had been used for a decade or two by Canton, Franklin and others. Maxwell (Introduction to Ref. 2) reconstructs the picture of the apparatus:

> It consisted of a pair of somewhat rickety wooden frames, to which two hemispheres of pasteboard were fastened by sticks of glass. By pulling a string these frames were made to open like a book, showing within the hemispheres of the memorable globe of 12.1 inches diameter, supported on a glass stick as axis. By pulling the string still more, the hemispheres were drawn quite away from the globes, and the pith-ball electrometer was drawn up to the globe to test its "degree of electrification." A machine so bulky, so brittle, and so inelegant was not likely to last long, even in a lumber room."

This last comment is a propos the fact that no trace of Cavendish's electrical apparatus was found (in 1879) in the Royal Institution, London, although there were still, extant, some of his chemical and meteorological equipment. Regarding these and the pith-ball electrometer, Maxwell remarks that:

> ...though Cavendish had a wonderful power of making correct observations and getting accurate results with these clumsy instruments, we must confess that in these, the most vital organs of electric research, Cavendish showed less inventive genius than some of his contemporaries... Cavendish did the best with the electrometers he found in existence, but he did not invent a better one. (loc. cit.)

Cavendish does however describe an ingenious modification of the <u>method of using</u> the pith-ball electrometer, which, he claims, make it a more sensitive detector for small charges.

Details of the electrometers used to determine the (absence of) charge of the inside sphere:

> The balls were made of pith of elder, turned round in a lathe, about one-fifth of an inch in diameter, and were

suspended by the finest linen threads that could be procured, about 9 inches long. (Ref.2 p. 119);

A reproducible degree of electrification on the outer hemisphere was established by use of a Henly Electrometer.

559] *Comparison of Henly's, Lane's, and straw electrometer.*

Sun. Mar. 28 [1773]. Th. about 58. N. about 8.

The two conductors of Nairne were placed end to end, and Henly's electrometer placed on that furthest from globe* parallel to conductor and the cork pointing from globe. The four jars were also joined to the usual wire with the straw electrometer hung to it, the wire and jars being placed at such a distance from the conductors that the electricity was found not to flow sensibly from them to the jars.

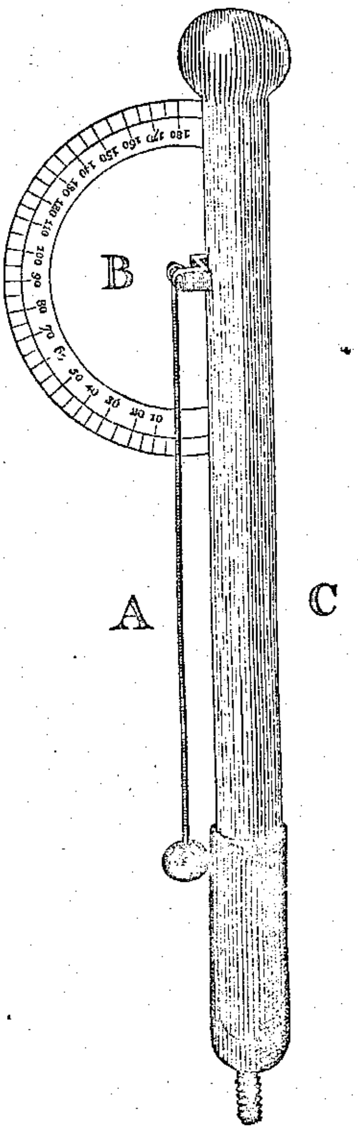
The globe 3† was then applied to that conductor nearest the globe and electrified till Henly's electrometer stood at 90°. The globe 3 was then removed from the conductors and its electricity communicated to the jars‡.

The straw electrometer separated to $2+\frac{1}{2}$.

The experiment was repeated several times and was found to agree together pretty well.

The jars were then electrified, they and the straw electrometer standing in the same place, and it was found that Lane's electrometer fastened to one of them discharged at $0{\cdot}53\frac{1}{4}$ with that degree of electrification, the same jar being applied to the conductor and electrified till Henly's electrometer stood at 90°, Lane's discharged at 12·15.

The conductors being then taken away and the jars and straw electrometer placed in usual position, Lane's discharged at 1·17 when straw stood at $2+3$, and at $1+2$ when light paper electrometer just separated. The knobs touched at 0·4.




[Henly's Electrometer, from the original figure, *Phil. Trans.* 1772, p. 359.]

* [Of Nairne's electrical machine.]

† [Globes 2 and 3 are glass globes coated as Leyden jars. See Art. 505 for their charges.]

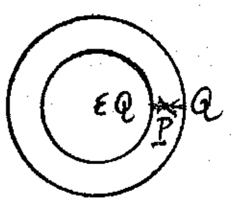
‡ [For the charges of these jars see Art. 506.]

Other particulars of the experiment are given in Cavendish's own account, and in the "Experimental Notes."

The analysis of the experiment given by Cavendish (Ref.2 pp.112-3) is based on the general principles and analytical procedures elaborated in his 1771 paper. The mathematics appears, by later standards, rather clumsy--a mixture of elementary geometry and Newtonian Fluxions. A completely general and formal analysis is not possible with such methods; but an essentially correct result is obtained by making judicious approximations, which are suggested by the results of the experiment, which indicates n is very close to 2.

Essentially, Cavendish computes--explicitly--the force at a point somewhere inside a spherical shell of charge for a $1/r^n$ law, (for n=2, this is, of course, precisely zero!). This is compared with the force due to the charge (very small--as experiment shows) on the inner sphere. For this latter, since n is approximately 2, he can reasonably equate the force to that of a point charge, located at the center and acting either with a $1/r^n$ of $1/r^2$ law--the difference is insignificant.

For equilibrium these two forces must balance when the inner and outer spheres are connected by a conducting wire. (Notice here that the wire is assumed not to disturb the spherical symmetry of the charge distribution. Could this have been justified?) Assume this happens when the charge on the inner sphere is E times that on the hemispheres.




Experimentally one observes that $\mathcal{E}$ is less than some small fraction: from the analysis this upper limit for $\mathcal{E}$ can be related to a maximum departure from 2 of n.

Equilibrium of forces at P due to charges $\mathcal{E}$Q on inner and Q on outer sphere.

Cavendish (Ref.2 p.112-113), working directly with the electric forces, obtains the result that the two forces will balance, at a point P distance d from the outer surface,

$$\mathcal{E}(1+p)(1-p)^{\delta}\, 2^{(1+\delta)} = \left(\frac{p^{-\delta}-p}{1+\delta} + \frac{p^{(1-\delta)}-1}{1-\delta}\right)$$

Here $p = d/(2r-d)$, where $2r$ is the diameter of the outer sphere. For $\delta = 0$, the R.H.S., and therefore E, is zero. Since, experimentally, we know $\delta \ll 1$, we can expand this expression for $\mathcal{E}$ in powers of $\delta$ ; which readily yields to first order in $\delta$ :

$$\mathcal{E} = \delta\left\{\frac{1}{2}\log_e\left(\frac{1}{p}\right) - \left(\frac{1-p}{1+p}\right)\right\}$$

In a sensitive arrangement, the quantity $\{\cdots\}$ should be large, so that a small departure of n from 2 (i.e. small $\delta$ ) would result in a large, detectible $\mathcal{E}$ . It is easily seen that a small value of p is desirable, viz.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| p: | 0.02 | 0.03 | 0.05 | o.10 | 0.2 | 0.5 | 1.0 |
| $\{\cdots\}$: approx. | 0.96 | 0.8 | 0.6 | 0.32 | 0.13 | 0.013 | 0 |

As p approaches zero, i.e. the radii of the inner and outer spheres become closer, $\{\cdots\}$ only increases logarithmically, and there are obviously practical limits to making the two spheres very nearly equal in size. In Cavendish's own experiment, d=0.35", 2r=13.1", so that p=0.0275 and $\{\cdots\}$ =0.85.*

Cavendish observes $\mathcal{E}$ less than 1/60th, from which he infers that $\delta$ is less than (1/60)/0.85 or approximately 1/50th. (This style of expressing results in vulgar fractions is again reminiscent of Newton!) He concludes:

> "...that the electric attraction and repulsion must be inversely as some power of the distance between that of 2 + 1/50 and 2-1/50, and there is no reason to think that it differs at all from the inverse duplicate ratio."

Faith in the inverse-square-law clearly transcends the limitations of experimental verification!

The assertion that the law is tested in this way both for attraction and repulsion invites some scrutiny. The one-fluid model is used by Cavendish. It is necessary to perform the experiment with both plus (excess, vitreous) and minus (deficit, resinous) charge on the outside?

In his 1771 paper Cavendish had demonstrated--not with complete, formal, conclusiveness--but to his own satisfaction however, that

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* There is some slight inconsistency between the dimensions Cavendish gives on p. 110 and on page 111 (ref. 2). However, this has little consequence since the value of $\{\cdots\}$ changes only slowly with p.

in a charged conductor of <u>any shape</u> whatsoever, the charge should reside in a thin surface layer. His unpublished description of the above experiment is followed, immediately, by a brief account of another experiment with a hollow, rectangular work box:

> A similar experiment was tried with a piece of wood 12 inches square and 2 inches thick, enclosed between two wooden drawers each 14 inches square and 2 inches deep on the outside, so as to form together a hollow box 14 inches square and 4 inches thick, the wood of which it was composed being .5 to .3 of an inch thick.
>
> The experiment was tried in just the same manner as the former. I could not perceive the inner box to be at all over or undercharged, which is a confirmation of what was supposed at the end of Prop. IX.--that when a body of any shape is overcharged, the redundant fluid is lodged entirely on the surface, supposing the electric attraction and repulsion to be inversely as the square(of the distance.
>
> (Ref. 2 p.112)

The theorem referred to is from the 1771 paper:

> 41] Prop. IX. If any body at a distance from any over or undercharged body be overcharged, the fluid within it will be lodged in greater quantity near the surface of the body than near the center. For, if you suppose it to be spread uniformly all over the body, a particle of fluid in it, near the surface, will be repelled towards the surface by a greater quantity of fluid than that by which it is repelled from it; consequently, the fluid will flow towards the surface, and make it denser there: moreover, the particles of fluid close to the surface will be pressed close together; for otherwise, a particle placed so near it, that the quantity of redundant fluid between it and the surface should be very small, would move towards it; as the small quantity of redundant fluid between it and the surface would be unable to balance the repulsion of that on the other side.
>
> From the four foregoing problems it seems likely, that if the electric attraction or repulsion is inversely as the square of the distance, almost all the redundant fluid in the body will be lodged close to the surface, and there pressed close together, and the rest of the body will be saturated. If the repulsion is inversely as some power of the distance between the square and the cube, it is likely that all parts of the body will be overcharged: and if it is inversely as some less power than the square, it is likely that all parts of the body, except those near the surface, will be undercharged.
>
> (Ref. 2 p.17)

The limitations of Cavendish's experiment are twofold: first, the limited sensitivity of the charge-detecting instrument he used (the two pith-ball electrometer); second, the possibility of some charge leaking down from the glass supporting rod. In both these respects a higher sensitivity is not too difficult to achieve even in the sort of technique available to Cavendish's time. But in principle the method is capable of extraordinary sensitivity, as we shall now see.

## VI. Postscript

When Clerk Maxwell edited Cavendish's unpublished papers-- in 1879-- the law-of inverse-squares for electric charges was taken as all but an axiomatic truth. Yet, up to that time, the direct experimental evidence in support of this law was little more, or better, than that which Cavendish had obtained 100 years earlier!

. Some direct measurements had, in the meantime, been made of the repulsion and attraction of fixed charges at varying distances-- notably those by Couloumb (c. 1884); but even a superficial glance at what is reported and claimed for these, removes any illusion that they represent--although often so described--a precise verification of the inverse square law. Their virtue is their directness--certainly not their precision, which does not attain, let alone exceed that of the Cavendish experiments.

At about the same time as Cavendish, Prof. John Robison (1739-1805), of Edinburgh, a distinguished and most versatile natural philosopher, observed that:

> ...in the mutual repulsion of two similarly electrified spheres, the law was slightly in excess of the inverse duplicate ratio of the distances, whilst in attraction of oppositely electrified spheres the deviation from that ratio was in defect. (Ref.8, p. 73)

But these observations seemed to have made little impact, or little influence that was acknowledged, at the time. In any event, by the mid-19th century the experimental basis of the inverse-square-law seemed to have been lost from sight. A quite sophisticated mathematical analysis of electricity had been built up in the previous few decades, by Poisson, Green, Gauss, Wm. Thomson (Kelvin) et al.; and in this the inverse-square law was assumed along with certain formalized properties of materials: insulator, conductor, which represented--in a somewhat abstract way--their real physical properties. Most of this abstraction implicitly assumes, and relies on, the validity of the inverse square law: with any other law ($n \neq 2$), these simple abstract properties do not suffice. For example, for a charged (solid) conductor, all the surplus (or deficit) charge in a "thin" surface layer, results in no electric force (field) inside, provided the inverse square law is valid. Consequently, we can postulate any amount of charge inside, provided only that there are equivalent amounts of both sorts (in the two-fluid: equal + and - sorts; in the one-fluid: corresponding amounts of fluid and ordinary matter-- "saturation"): in the absence of an internal field the balance will not be destroyed. But for $n \neq 2$ this simple situation is absent: there is some internal force and it will interact with the "fluids". The final "equilibrium" in this case will therefore depend on additional physical assumptions: e.g. the maximum "fluid" that can be displaced; the ultimate density of fluid, etc. It seems then more than likely that the inverse-square-law was welcome, or accepted without murmur, just because it made possible a formal theory--the very theory of "classical" electrostatics which we use today. Cavendish put it modestly when he concluded that there was no reason to think otherwise; 100 years later there was a great vested interest in this law!

None the less, it is an experimental law, and subject to test; and this is just what Maxwell did at the time (1879) when he was editing Cavendish's papers. The principle used is identical with Cavendish's; the technique is greatly superior. Maxwell identified clearly the limitations of the earlier experiment--the limited sensitivity, and the possible electrical "leakage" through the insulating supports of the inner cylinder he overcame both, and achieved the remarkable result, about 400 times more precise than Cavendish's: $|n-2| < 1/21{,}600$. (See Ref. 2, p. 417-419; Appendix, p.58)

In the analysis of his experiment Maxwell, using contemporary potential theory, is able to avoid the simplifying approximations that Cavendish used. But in the end, since n is very close to 2, an expansion in $\delta$ (= n-2) is made; so that the more refined analysis is--elegance apart--not really material.

Laplace gave the first formal proof that the inverse-square is the only law consistent with the result that a uniform spherical shell (of charge) exerts no force in the interior (Ref. 2, p. 422--appended). Maxwell comments on this proof that:

> ...though the assumption of Cavendish, that the force varies as some inverse power of the distance appears less general than that of Laplace, who supposes it to be any function of the distance, it is the most general assumption which makes the ratio

> of the force at two different distances a function of the ratio of those distances.
>
> If the law of force is not a power of the distance, the ratio of the forces at two different distances is not a function of the ratio of the distances alone, but also of one or more linear parameters, the values of which if determined by experiment would be absolute physical constants, such as might be employed to give us an invariable standard of length.
>
> Now although absolute physical constants occur in relation to all the properties of matter, it does not seem likely that we should be able to deduce a linear constant from the properties of anything so little like ordinary matter as electricity appears to be. (Ref. 2, p. 422)

Here, with his usual clarity and insight, Maxwell anticipates one aspect of the law-of-force--its "range", which was to assume great significance some 50 years later, and still retains a deep interest.

The inverse-square law--in the form given it by Gauss, Laplace and Poisson can be written: $\nabla^2 V=0$, $V=q/r$, where V is the static potential of a "point charge," and elsewhere the space is empty. There is no characteristic "range" for this force. But if the potential were to have some characteristic range, for example: $q\frac{\exp-(r/r_0)}{r}$, the Laplace-Poisson equation is no longer valid; instead-- $\nabla^2 V + \frac{1}{r_0^2} V = 0$.

Electric forces--and with proper generalizations, electromagnetic forces--are not, then, characterized by a "range" and can be expressed in characteristically (simple!) mathematical form. But in the first two or three decades of the present century, it became clear that there were "forces"--those binding together the constituents of the nucleus,--which did seem to have a definite range, in this case a very short one ($\sim 10^{-13}$). Yukawa (in 1936), speculating on the significance of this, conjectured that the inverse-square law was associated with the zero (rest) mass of an intermediary (the photon of the electromagnetic field) responsible for electric forces. An intermediary field of non-zero rest mass of an intermediary (the photon of the electromagnetic field) responsible for electric forces. An intermediary field of non-zero rest mass would "automatically" possess a finite range, the relation between range $r_0$ and mass m being simply $r_0 \sim 1/m$. This is the famous Yukawa-potential $V\frac{e^{-r/r_0}}{r}$ of nuclear-meson physics.

Yukawa's seminal idea gave rise to the whole modern concept of the interaction of two particles being mediated by some other "particle-field"; electromagnetic interaction is a very "special" (but immensely important) example of a "zero-rest mass" field, one which has, therefore, an inverse-square law dependence on distance. (Gravity--regarded as a field--is another example of a zero-mass field.)

In this "modern" spirit it is fashionable to consider any possible departure from the inverse-square law in terms of a "non-zero" rest mass (for the photon). It is also in the spirit of contemporary physics to suspect the absolute truth of any generalization. Has not Newton's mechanics been shown only to be an approximation to the "true" relativistic mechanics? Classical "laws" from the viewpoint of quantum-mechanics are also reduced to special limiting approximations. More recently such "absolute" principles as left-right (parity) symmetry and charge symmetry have been shown not to have unlimited validity. In this spirit, any "ideal" law may be placed under suspicion--or better still, subject to the fullest experimental scrutiny to test (or at least extend) its limits of validity. And as experimental techniques develop rapidly, new methods enable the range of validity of "old laws" to be greatly extended, often by remarkably large factors.

Maxwell improved on Cavendish by a factor of 4,000. The next major assault on the inverse-square law, by Plimpton and Lawton (1936) (see appendix), still using the basic Cavendish principle, in a paper entitled "Test of Couloumb's Law", achieved a further factor of 20,000!

More recently, and now under the banner of setting a limit to the "rest-mass-of-the-photon" the limit has been pushed back a further factor of about one million" i.e.

In the more exotic language of photon-rest-mass, this sets an upper limit to such a mass of about $2 \times 10^{-47}$ gm., or nearly $10^{20}$ times less than the electron rest mass.

The enormous increase of sensitivity in the experiment has been possibly essentially on account of the vastly superior techniques now available for detecting small electrical signals. Inter-alia, these permit the entire detecting equipment to be located inside the equivalent of Cavendish's inner-sphere, so that stray leakage charges can be entirely eliminated. (See Appendix iv; Note 19) There is some indirect evidence, from electrical phenomena on a terrestrial and planetary scale, for an even smaller limit to the "photon-rest mass": there is no evidence that it is not zero.

Cavendish's conclusion, that "there is no reason to think that it (the law of fource) differs at all from the inverse-duplicate-ratio" still stands.

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8. Paul Fleury Mottelay: Bibliographical History of Electricity and Magnetism London, 1922. (up to 1821)

Textbooks:

9. E. M. Purcell: Electricity and Magnetism. (A clear and authoritative account of the elementary aspect of the subject from a contemporary viewpoint.)

10. J.C. Maxwell: An Elementary Treatise on Electricity, Oxford, Clarendon Press, 1881. (Chap. I, II, IV--an especially clear and elementary exposion of the principles)

    Treatise on Electricity and Magnetism, Vol. 1, Oxford Clarendon Press, 1873. (Especially Chap. I, II for principles; and Chap. III, IV for analytical methods.)

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Other Reports: (Barnard-Columbia History of Physics)
Benjamin Franklin and the conservation of Electricity.
Charles Couloumb and the Measurement of Electric Charge.

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*A separate offprint of the 1771 Paper is available.

Dramatis Personae

Franz Ulrich Aepinus (1724-1802): Professor of Astronomy at Berlin, Superintendant of Normal School, St. Petersburgh.

Jean le Rond D'Alembert (1717-1783): French academician in Paris.

Daniel Bernoulli (1700-1782): Swiss mathematician.

Jean (Jacob) Bernoulli (1667-1748): Professor of Mathematics at Groningan and Basel.

George Buffon (1707-1788): French Naturalist.

John Canton (1718-1772): English experimental Philosopher, Fellow of Royal Society.

Charles Augustin Coulomb (1736-1806): French military engineer, academician etc. (Paris).

Lord Charles Cavendish (1703?-1783): Fellow of Royal Society, London.

Henry Cavendish (1731-1810): his son.

Charles Francois Du Fay (1698-1739): Superintendent of Gardens to the King of France, Paris.

Leonhard Euler (1707-1783): Professor of Mathematics at the St. Petersburg Academy and at Prussian academy of Sciences, Berlin.

Michael Faraday (1791-1718): Assistant, later Director of Royal Institute, London.

Benjamin Franklin (1706-1790): A printer from Philadelphia.

Carl Friedrich Gauss (1777-1855): Professor of Mathematics, Gottingen.

George Green(1793-1841): English mathematician.

William T. Henley (-- 1779) Fellow of Royal Society.

Frederick William Herschel (1738-1822): Musician, astronomer to George III, England.

Christian Huygens (1629-1695): Dutch physicist and astronomer.

Johann Kepler (1571-1630): Astronomer to Emperor Rudolf II and Emperor Matthias, Prague.

Ebenezer Kinnersley (1711-1778): An "unemployed" Baptist minister.

Joseph LaGrange (1736-1813): Professor of Mathematics, Paris.

Pierre-Simon Lapace (1749-1827): French Mathematician, philosopher.

Antoine-Laurent Lavoisier (1743-1794): French chemist and natural philosopher, Paris.

James Clerk Maxwell (1831-1879): Professor of Natural philosophy, Aberdeen, London; Experimental physics, Cambridge.

John Michell (1724-1793): English geologist.

Georg Simon Ohm (1787-1854): German physicist.
Simeon D. Poisson (1781-1840): Prof. of Mechanique Rationelle, Paris.
Joseph Priestley (1733-1804): Nonconformist minister and school teacher, Yorkshire, England.

John Robison (1739-1805): Professor of Natural Philosophy, Edinburgh; St. Petersburg.

Count Rumford (Benjamin Thompson) (1753-1814): American-British scientist and adventurer.

William Snow-Harris (1781-1867): English physician and electrician.

Robert Symmers (-- 1763): Fellow of Royal Society, London.

William Thomson (Kelvin) (1824-1907): Scottish mathematician and physicist.

Alexander Volta (1745-1827): Professor at Como, and Pavia, Italy.

William Watson (1715-1787): A London Apothecary and physician; fellow of Royal Society.

James Watt (1736-1819): Scottish engineer.

Johann Wilke (1732-1796): Professor of Experimental physics at Military Academy; secretary of Academy of Science, Stockholm.

Benjamin Wilson (1708-1788): Fellow of Royal Society, London.

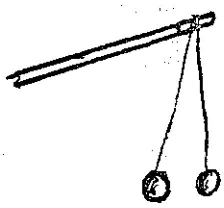
## VIII Experimental Notes

(Paragraph references ) are to the Cavendish unpublished account. Appendix 3. p. 52

1. The main experiment to be performed is a reproduction of Cavendish's determination of the "law of Electric Force"; Cavendish's own account (unpublished, until 1879) is appended. The reconstructed apparatus is essentially the same as the original in all matters of procedure and technique. With suitable refinements, this method is, in principle, capable of extraordinary sensitivity (c.f. Chap. VI above ). You may make small modifications and improvements in procedure as you think appropriate. But to keep these meaningful they should be of a type which might have been considered by Cavendish using the techniques of his day. In any event you should aim to make experiments that are as convincing and of a precision at least comparable with Cavendish's. The experiment is a quantitative one (although the essential quantity to be measured is zero or close to zero); and Cavendish treats it as such. He is very conscious of possible sources of error and makes tests to verify their absence or modifications to eliminate them. You should emulate him!

2. Before attempting the main experiment, it will be well to gain some familiarity with the techniques and instruments used. These were developed in the period 1750-1770 and Cavendish had clearly mastered them.

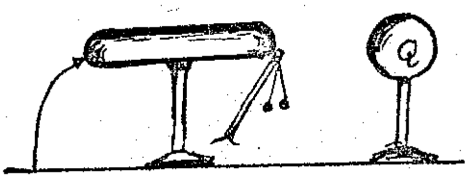
i) The chief "instrument" is Canton's pith-ball (Ref. 10) electroscope: two small light conducting spheres are suspended from a common point by conducting threads (about 4"-6" long should prove convenient). They are supported from an insulated (lucite/glass) rod. Observe their response to charge (q), as it is brought up from some distance (Ref. 2 p. 50) Now electrify the pith-balls by contact with a large charged object (Q), and again observe the reaction as the charge (q) is brought up.



ii) a similar pair of pith-balls should be assembled using insulating (silk/nylon) threads, in place of the conducting (thin wire/ damp thread) threads used in (i). Repeat the experiments in (i), noticing the difference.

iii) The use of the pith-ball electroscope is well exemplified in the study of electric induction (Canton, Aepinus etc. 1750-1760), the theory of which is thoroughly examined by Cavendish.

A long cylindrical conductor is mounted on an insulated support. A charged object is brought near one end.

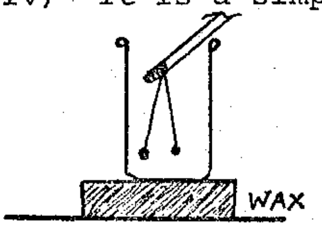



The electrification of various points on the cylinder can be studied. If the electroscope is slightly charged before it is used to explore the cylinder, then the "sign" of the electrification can also be determined.

It will be observed that some parts of the cylinder are electrified oppositely to others. Where is the region separating these two zones? (Lord Stanhope, c. 1750, inferred the law-of-force from the location of this zone.)

How does the electrification change when the cylinder is connected to "ground" by a fine wire. Examine (and exlain!) this effect for a variety of "grounding" points (on the cylinder).

iv) It is a simple matter to repeat the Franklin-Priestly experiment. (Notes pp. 19/20). Use a can, of ample size, placed on wax. Charge it by means of the electrostatic machine.




v) Some tests should be made--if not already done--of the quality of typical insulation on the actual day in which you intend to repeat Cavendish's experiments.

vi) A simple auxiliary ("gold-leaf"--but thin aluminum foil is as good, or better) electroscope may be constructed and its sensitivity compared with the pith-ball electroscope. What essential features determine the sensitivity?

3. The Cavendish Experiment

i) First test the insulation (i.e. how long charge may be retained) on both the inner sphere and outer hemispheres; the former for small charges, the latter for large charges--as will be significant in the experiment. Judge whether the insulation is adequate for the measurement you propose to make.

ii) Cavendish's procedure is summarized in 220], 222], pp. 105-107. Appendix p.52. If the insulation of the outer hemispheres is "poor," the order of the operations

a) removing the wire connecting (momentarily) the inner and outer conductors,
b) disconnecting the connection to the Leyden Jar

may be reversed. (What are the advantages and disadvantages of the two orders of procedure?)

But the connecting wire MUST be removed before the hemispheres are opened. (WHY?)

iii) In any event the outer sphere should be electrified for as short a time as possible(Cf.223] Why? Notice 226]. Cavendish stresses that the whole cycle of operations be made as quickly as possible. But it may (and should) be repeated many times, varying any feature which is suspected of causing some disturbance or limiting the accuracy.

iv) The experiment is repeated with the pith-ball electrometer given an initial positive/negative charge. (Cf. (1) above.)

v) Calibration of the sensitivity is made as in 230]. Cavendish--in other unpublished papers--gives detailed descriptions of techniques and measurements for comparison of two condensors or capacitors (as they are now termed). The following is one such method which may be reproduced without difficulty.

Charge the large jar and observe its state of electrification with the electrometer. Now connect it to the smaller jar (by a fine wire-- why must it be fine?); discharge the smaller jar; repeat the whole operation n-times--until the electrometer shows an "electrification" equal to (say) half the original.

If C is the capacity of the large jar and c of the smaller one, then $\left(\frac{C}{C+c}\right)^n = \frac{1}{2}$, i.e. $\frac{c}{C} = \sqrt[n]{2} - 1$. (This is readily proved!)

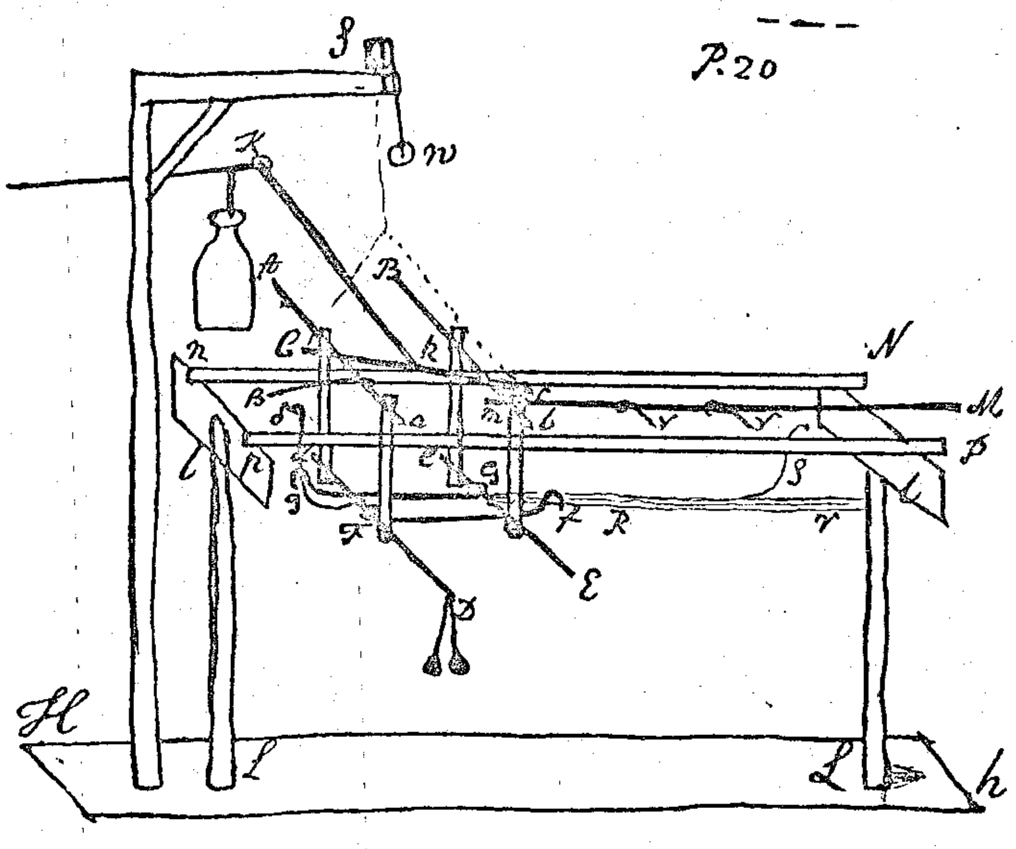
It is a simple matter to devise a method of showing when the electrometer readings indicate a factor 2 in electrification. (Devise such a procedure.)

vi) If the "gold-leaf" electroscope constructed (cf.2, vi above) is more sensitive than the pith-ball electroscope, you can repeat the experiment with the former, and check whether the limiting feature is the electroscope sensitivity--or some other feature.

4. Analysis and Interpretation. See Cavendish's paper, Appendix. p.52 : 234], 235]; and Notes. Chapter 5. Also: Appendix p. 58.

This investigation should conclude with an assessment of the limiting features of Cavendish's experiment. How far might the use of an electroscope of much higher sensitivity reduce the difference between n and 2 that could be detected? (What is the relevant sensitivity of such an electroscope--the "charge-" or "potential-" sensitivity?)

Maxwell's improved version of Cavendish's experiment is an example of such a development. If a suitable electrometer (e.g. a small Quadrant electrometer) is available, it is not difficult to reproduce Maxwell's far more sensitive experiment. (See Appendix p.58 )




Typical Sketch of Cavendish's Electrical Apparatus.
(From Ref. 2)

Appendix 1

Extracts from Newton's 'Principia' Book I, relating to the Inverse Square Law.
(From Motte's 1729 Translation. Revised E. Cajori. Univ. of California Press 1971. pp 193-196;214-215)

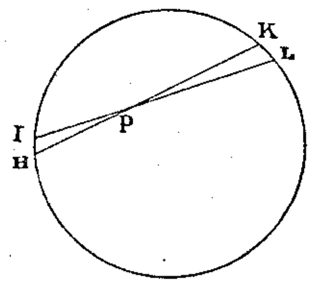
## SECTION XII

*The attractive forces of spherical bodies.*

### PROPOSITION LXX. THEOREM XXX

*If to every point of a spherical surface there tend equal centripetal forces decreasing as the square of the distances from those points, I say, that a corpuscle placed within that surface will not be attracted by those forces any way.*

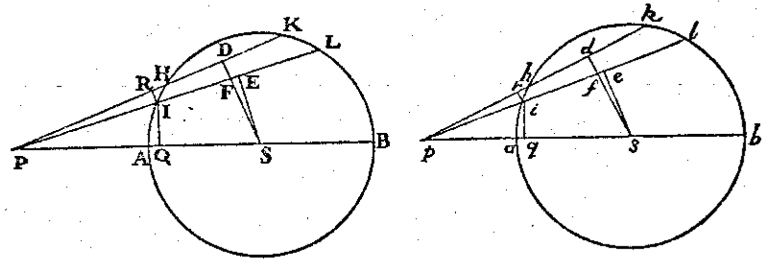
Let HIKL be that spherical surface, and P a corpuscle placed within. Through P let there be drawn to this surface two lines HK, IL, intercepting very small arcs HI, KL; and because (by Cor. III, Lem. VII) the triangles HPI, LPK are alike, those arcs will be proportional to the distances HP, LP; and any particles at HI and KL of the spherical surface, terminated by right lines passing through P, will be as the square of those distances. Therefore the forces of these particles exerted upon the body P are equal between themselves. For the forces are directly as the particles, and inversely as the square of the distances. And these two ratios compose the ratio of equality, 1 : 1. The attractions therefore, being equal, but exerted in opposite directions, destroy each other. And by a like reasoning all the attractions through the whole spherical surface are destroyed by contrary attractions. Therefore the body P will not be any way impelled by those attractions. Q.E.D.




### PROPOSITION LXXI. THEOREM XXXI

*The same things supposed as above, I say, that a corpuscle placed without the spherical surface is attracted towards the centre of the sphere with a force inversely proportional to the square of its distance from that centre.*

Let AHKB, *ahkb* be two equal spherical surfaces described about the centres S, *s*; their diameters AB, *ab*; and let P and *p* be two corpuscles situate without the spheres in those diameters produced. Let there be drawn from the corpuscles the lines PHK, PIL, *phk*, *pil*, cutting off from the great circles AHB, *ahb*, the equal arcs HK, *hk*, IL, *il*; and to those lines let fall the perpendiculars SD, *sd*, SE, *se*, IR, *ir*; of which let SD, *sd*, cut PL, *pl*, in F and *f*.




Let fall also to the diameters the perpendiculars IQ, *iq*. Let now the angles DPE, *dpe* vanish; and because DS and *ds*, ES and *es* are equal, the lines PE, PF, and *pe*, *pf*, and the short lines DF, *df* may be taken for equal; because their last ratio, when the angles DPE, *dpe* vanish together, is the ratio of equality. These things being thus determined, it follows that

$$PI : PF = RI : DF$$

and $\quad pf : pi = df$ or $DF : ri$.

Multiplying corresponding terms,

$$PI \cdot pf : PF \cdot pi = RI : ri = \text{arc } IH : \text{arc } ih \text{ (by Cor. III, Lem. VII)}.$$

Again, $\quad PI : PS = IQ : SE$

and $\quad ps : pi = se$ or $SE : iq$.

Hence, $\quad PI \cdot ps : PS \cdot pi = IQ : iq$.

Multiplying together corresponding terms of this and the similarly derived preceding proportion,

$$PI^2 \cdot pf \cdot ps : pi^2 \cdot PF \cdot PS = HI \cdot IQ : ih \cdot iq,$$

that is, as the circular surface which is described by the arc IH, as the semicircle AKB revolves about the diameter AB, is to the circular surface described by the arc *ih* as the semicircle *akb* revolves about the diameter *ab*. And the forces with which these surfaces attract the corpuscles P and *p* in the direction of lines tending to those surfaces are directly, by the hypothesis, as the surfaces themselves, and inversely as the squares of the distances of the surfaces from those corpuscles; that is, as $pf \cdot ps$ to $PF \cdot PS$. And these forces again are to the oblique parts of them which (by the resolution of forces as in Cor. II of the Laws) tend to the centres in the directions of the lines PS, *ps*, as PI to PQ, and *pi* to *pq*; that is (because of the like triangles PIQ and PSF, *piq* and *psf*), as PS to PF and *ps* to *pf*. Thence, the attraction of the corpuscle P towards S is to the attraction of the corpuscle *p* towards *s* as $\frac{PF \cdot pf \cdot ps}{PS}$ is to $\frac{pf \cdot PF \cdot PS}{ps}$, that is, as $ps^2$ to $PS^2$. And, by a like reasoning, the forces with which the surfaces described by the revolution of the arcs KL, *kl* attract those corpuscles, will be as $ps^2$ to $PS^2$. And in the same ratio will be the forces of all the circular surfaces into which each of the spherical surfaces may be divided by taking *sd* always equal to SD, and *se* equal to SE. And therefore, by composition, the forces of the entire spherical surfaces exerted upon those corpuscles will be in the same ratio. Q.E.D.

## PROPOSITION LXXII. THEOREM XXXII

*If to the several points of a sphere there tend equal centripetal forces decreasing as the square of the distances from those points; and there be given both the density of the sphere and the ratio of the diameter of the sphere to the distance of the corpuscle from its centre: I say, that the force with which the corpuscle is attracted is proportional to the semidiameter of the sphere.*

For conceive two corpuscles to be severally attracted by two spheres, one by one, the other by the other, and their distances from the centres of the spheres to be proportional to the diameters of the spheres respectively; and the spheres to be resolved into like particles, disposed in a like situation to the corpuscles. Then the attractions of one corpuscle towards the several particles of one sphere will be to the attractions of the other towards as many analogous particles of the other sphere in a ratio compounded of the ratio of the particles directly, and the square of the distances inversely. But the particles are as the spheres, that is, as the cubes of the diameters, and the distances are as the diameters; and the first ratio directly with the last ratio taken twice inversely, becomes the ratio of diameter to diameter. Q.E.D.

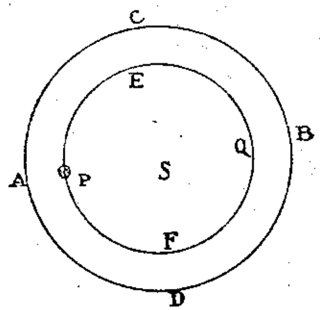
Cor. I. Hence if corpuscles revolve in circles about spheres composed of matter equally attracting, and the distances from the centres of the spheres be proportional to their diameters, the periodic times will be equal.

Cor. II. And, *vice versa,* if the periodic times are equal, the distances will be proportional to the diameters. These two Corollaries appear from Cor. III, Prop. IV.

Cor. III. If to the several points of any two solids whatever, of like figure and equal density, there tend equal centripetal forces decreasing as the square of the distances from those points, the forces, with which corpuscles placed in a like situation to those two solids will be attracted by them, will be to each other as the diameters of the solids.

## PROPOSITION LXXIII. THEOREM XXXIII

*If to the several points of a given sphere there tend equal centripetal forces decreasing as the square of the distances from the points, I say, that a corpuscle placed within the sphere is attracted by a force proportional to its distance from the centre.*




In the sphere ACBD, described about the centre S, let there be placed the corpuscle P; and about the same centre S, with the interval SP, conceive described an interior sphere PEQF. It is plain (by Prop. LXX) that the concentric spherical surfaces of which the difference AEBF of the spheres is composed, have no effect at all upon the body P, their attractions being destroyed by contrary attractions. There remains, therefore, only the attraction of the interior sphere PEQF. And (by Prop. LXXII) this is as the distance PS. Q.E.D.

### SCHOLIUM

By the surfaces of which I here imagine the solids composed, I do not mean surfaces purely mathematical, but orbs so extremely thin, that their thickness is as nothing; that is, the evanescent orbs of which the sphere will at last consist, when the number of the orbs is increased, and their thickness diminished without end. In like manner, by the points of which lines, surfaces, and solids are said to be composed, are to be understood equal particles, whose magnitude is perfectly inconsiderable.

## PROPOSITION LXXIV. THEOREM XXXIV

*The same things supposed, I say, that a corpuscle situated without the sphere is attracted with a force inversely proportional to the square of its distance from the centre.*

For suppose the sphere to be divided into innumerable concentric spherical surfaces, and the attractions of the corpuscle arising from the several surfaces will be inversely proportional to the square of the distance of the corpuscle from the centre of the sphere (by Prop. LXXI). And, by composition, the sum of those attractions, that is, the attraction of the corpuscle towards the entire sphere, will be in the same ratio. Q.E.D.

## SECTION XIII

*The attractive forces of bodies which are not spherical.*

### PROPOSITION LXXXV. THEOREM XLII

*If a body be attracted by another, and its attraction be vastly stronger when it is contiguous to the attracting body than when they are separated from each other by a very small interval; the forces of the particles of the attracting body decrease, in the recess of the body attracted, in more than the squared ratio of the distance of the particles.*

For if the forces decrease as the square of the distances from the particles, the attraction towards a spherical body being (by Prop. LXXIV) inversely as the square of the distance of the attracted body from the centre of the sphere, will not be sensibly increased by the contact, and it will be still less increased by it, if the attraction, in the recess of the body attracted, decreases in a still less proportion. The Proposition, therefore, is evident concerning attractive spheres. And the case is the same of concave spherical orbs attracting external bodies. And much more does it appear in orbs that attract bodies placed within them, because there the attractions diffused through the cavities of those orbs are (by Prop. LXX) destroyed by contrary attractions, and therefore have no effect even in the place of contact. Now if from these spheres and spherical orbs we take away any parts remote from the place of contact, and add new parts anywhere at pleasure, we may change the figures of the attractive bodies at pleasure; but the parts added or taken away, being remote from the place of contact, will cause no remarkable excess of the attraction arising from the contact of the two bodies. Therefore the Proposition holds good in bodies of all figures. Q.E.D.

Cor. I. Hence the attractions of homogeneous spheres at equal distances from the centres will be as the spheres themselves. For (by Prop. LXXII) if the distances be proportional to the diameters of the spheres, the forces will be as the diameters. Let the greater distance be diminished in that ratio; and the distances now being equal, the attraction will be increased as the square of that ratio; and therefore will be to the other attraction as the cube of that ratio; that is, in the ratio of the spheres.

Cor. II. At any distances whatever the attractions are as the spheres applied to the squares of the distances.

Cor. III. If a corpuscle placed without an homogeneous sphere is attracted by a force inversely proportional to the square of its distance from the centre, and the sphere consists of attractive particles, the force of every particle will decrease as the square of the distance from each particle.

### PROPOSITION LXXXVI. THEOREM XLIII

*If the forces of the particles of which an attractive body is composed decrease, in the recession of the attractive body, as the third or more than the third power of the distance from the particles, the attraction will be vastly stronger in the point of contact than when the attracting and attracted bodies are separated from each other, though by ever so small an interval.*

For that the attraction is infinitely increased when the attracted corpuscle comes to touch an attracting sphere of this kind, appears, by the solution of Problem XLI, exhibited in the second and third Examples. The same will also appear (by comparing those Examples and Theor. XLI together) of attractions of bodies made towards concavoconvex orbs, whether the attracted bodies be placed without the orbs, or in the cavities within them. And by adding to or taking from those spheres and orbs any attractive matter anywhere without the place of contact, so that the attractive bodies may receive any assigned figure, the Proposition will hold good of all bodies universally. Q.E.D.

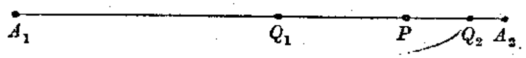
Maxwell's discussion of distribution of
charge for n≠2
Ref. 2 pp 368-372

NOTE 2, ARTS. 27 AND 282.

The problem of the distribution, in a sphere or ellipsoid, of a fluid, the particles of which repel each other with a force varying inversely as the $n^{th}$ power of the distance, has been solved by Green*. Green's method is an extremely powerful one, and allows him to take account of the effect of any given system of external forces in altering the distribution.

If, however, we do not require to consider the effect of external forces, the following method enables us to solve the problem in an elementary manner. It consists in dividing the body into pairs of corresponding elements, and finding the condition that the repulsions of corresponding elements on a given particle shall be equal and opposite.

(1) *Specification of Corresponding Points on a line.*




Let $A_1A_2$ be a finite straight line, let $P$ be a given point in the line, and let $Q_1$ and $Q_2$ be corresponding points in the segments $A_1P$ and $PA_2$ respectively, the condition of correspondence being

$$\frac{1}{Q_1P} - \frac{1}{A_1P} = \frac{1}{PQ_2} - \frac{1}{PA_2}. \tag{1}$$

It is easy to see that when $Q_1$ coincides with $A_1$, $Q_2$ coincides with $A_2$, and that as $Q_1$ moves from $A_1$ to $P$, $Q_2$ moves in the opposite direction from $A_2$ to $P$, so that when $Q_1$ coincides with $P$, $Q_2$ also coincides with $P$.

Let $Q_1'$ and $Q_2'$ be another pair of corresponding points, then

$$\frac{1}{Q_1'P} - \frac{1}{A_1P} = \frac{1}{PQ_2'} - \frac{1}{PA_2}. \tag{2}$$

Subtracting (1) from (2)

$$\frac{1}{Q_1'P} - \frac{1}{Q_1P} = \frac{1}{PQ_2'} - \frac{1}{PQ_2}, \tag{3}$$

or

$$\frac{Q_1Q_1'}{Q_1'P \,.\, Q_1P} = \frac{Q_2'Q_2}{PQ_2' \,.\, PQ_2}. \tag{4}$$

If the points $Q_1$ and $Q_1'$ are made to approach each other and ultimately to coincide, $Q_1Q_1'$ ultimately becomes the fluxion of $Q$, which we may write $\dot{Q}_1$, and we have

$$\frac{\dot{Q}_1}{Q_1P^2} = \frac{\dot{Q}_2}{PQ_2^2}, \tag{5}$$

or corresponding elements of the two segments are in the ratio of the squares of their distances from $P$.

Let us now suppose that $A_1PA_2$ is a double cone of an exceedingly small aperture, having its vertex at $P$; let us also suppose that the density of the redundant fluid at $Q_1$ is $\rho_1$, and at $Q_2$ is $\rho_2$; then since the areas of the sections of the cone at $Q_1$ and $Q_2$ are as the squares of the distances from $P$, and since the lengths of corresponding elements are also, by (5), as the squares of their distances from $P$, the quantities of fluid in the two corresponding elements at $Q_1$ and $Q_2$ are as $\rho_1Q_1P^4$ to $\rho_2PQ_2^4$. If the repulsion is inversely as the $n^{th}$ power of the distance, the condition of equilibrium of a particle of the fluid at $P$ under the action of the fluid in the two corresponding elements at $Q_1$ and $Q_2$ is

$$\rho_1Q_1P^{4-n} = \rho_2PQ_2^{4-n}. \tag{6}$$

We have now to show how this condition may be satisfied by one and the same distribution of the fluid when $P$ is any point within an ellipsoid or a sphere. We must therefore express $\rho$ so that its value is independent of the position of $P$.

Transposing equation (1) we find—

$$\frac{1}{Q_1P} + \frac{1}{PA_2} = \frac{1}{PQ_2} + \frac{1}{A_1P}. \tag{7}$$

Multiplying the corresponding members of equations (1) and (7) and omitting the common factor $A_1P \,.\, PA_2$,

$$\frac{A_1Q_1 \,.\, Q_1A_2}{Q_1P^2} = \frac{A_1Q_2 \,.\, Q_2A_2}{PQ_2^2}, \tag{8}$$

we may therefore write, instead of equation 6,

$$\rho_1(A_1Q_1 \,.\, Q_1A_2)^{\frac{4-n}{2}} = \rho_2(A_1Q_2 \,.\, Q_2A_2)^{\frac{4-n}{2}}. \tag{9}$$

Let us now suppose that $A_1A_2$ is a chord of the ellipsoid, whose equation is

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1. \tag{10}$$

If we write

$$1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} - \frac{z^2}{c^2} = p^2, \tag{11}$$

then the product of the segments of the chord at $Q_1$ is to the product of the segments at $Q_2$ as the values of $p^2$ at these points respectively, or

$$A_1Q_1 \,.\, Q_1A_2 : A_1Q_2 \,.\, Q_2A_2 :: p_1^2 : p_2^2. \tag{12}$$

* "Mathematical Investigations concerning the laws of the equilibrium of fluids analogous to the electric fluid, with other similar researches," *Transactions of the Cambridge Philosophical Society*, 1833. Read Nov. 12, 1832. See Mr Ferrers' Edition of Green's Papers, p. 119.

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We may therefore write, instead of equation (9),

$$\rho_1 p_1^{4-n} = \rho_2 p_2^{4-n}. \tag{13}$$

If, therefore, throughout the ellipsoid,

$$\rho = C p^{n-4}, \tag{14}$$

where $C$ is constant, every particle of the fluid within the ellipsoid will be in equilibrium.

We have in the next place to determine whether a distribution of this kind is physically possible.

Let $E$ be the quantity of redundant fluid in the ellipsoid,

$$E = C\int_0^1 p^{n-4}\, 4\pi abc\, p\,(1-p^2)^{\frac{1}{2}}\, dp$$

$$= 4\pi abc\, C\int_0^1 p^{n-3}(1-p^2)^{\frac{1}{2}}\, dp \tag{15}$$

$$= 2\pi abc\, C\, \frac{\Gamma\left(\frac{3}{2}\right)\Gamma\left(\frac{n-2}{2}\right)}{\Gamma\left(\frac{n+1}{2}\right)}. \tag{16}$$

Let $\rho_0$ be the density of the redundant fluid if it had been uniformly spread through the volume of the ellipsoid, then

$$E = \frac{4\pi}{3} abc\, \rho_0, \tag{17}$$

and if $\rho$ is the actual density of the redundant fluid,

$$\rho = \rho_0\, \frac{2}{3}\, \frac{\Gamma\left(\frac{n+1}{2}\right)}{\Gamma\left(\frac{3}{2}\right)\Gamma\left(\frac{n-2}{2}\right)}\, p^{n-4}. \tag{18}$$

When $n$ is not less than 2, there is no difficulty about the interpretation of this result.

The density of the redundant fluid is everywhere positive.

When $n = 4$ it is everywhere uniform and equal to $\rho_0$.

When $n$ is greater than 4 the density is greatest at the centre and is zero at the surface, that is to say, in the language of Cavendish, the matter at the surface is saturated.

When $n$ is between 2 and 4 the density of the redundant fluid at the centre is positive and it increases towards the surface. At the surface itself the density becomes infinite, but the quantity collected on the surface is insensible compared with the whole redundant fluid.



**When $n$ is equal to 2, $\Gamma\left(\frac{n-2}{2}\right)$ becomes infinite, and the value of $\rho$ is zero for all points within the ellipsoid, so that the whole ch. rge is collected on the surface, and the interior parts are exactly saturated, and this we find to be consistent with equilibrium.**

When $n$ is less than 2 the integral in equation (15) becomes infinite. Hence if we assume a value for $C$ in the interior parts of the ellipsoid, we cannot extend the same law of distribution to the surface without introducing an infinite quantity of redundant fluid. We might therefore conclude that if the quantity of redundant fluid is given, we must make $C = 0$, and suppose the redundant fluid to be all collected at the surface, and the interior to be exactly saturated. But, on trying this distribution, we find that it is not consistent with equilibrium. For when $n$ is less than 2, the effect of a shell of fluid on a particle within it is a force directed from the centre. If, therefore, a sphere of saturated matter is surrounded by a shell of electric fluid, the fluid in the sphere will be drawn towards the shell, and this process will go on till the different parts of the interior of the sphere are rendered undercharged to such a degree that each particle of fluid in the sphere is as much attracted to the centre by the matter of the sphere as it is repelled from it by the fluid in the sphere and the shell together. This is the same conclusion as that stated by Cavendish.

Green solves the problem, on the hypothesis of two fluids, in the following manner.

Suppose that the sphere, when saturated, contains a finite quantity, $E$, of the positive fluid, and an equal quantity of the negative fluid, and let a quantity, $Q$, of one of them, say the positive, be introduced into the sphere.

Let the whole of the positive fluid be spread uniformly over the surface of the sphere whose radius is $a$, so that if $P'$ is the surface-density,

$$4\pi a^2 P' = E + Q.$$

Green then considers the equilibrium of fluid in an inner and concentric sphere of radius $b$, acted on by the fluid in the surface whose radius is $a$, and shows that if the density of the fluid is

$$\rho = \frac{2}{\pi} P' a \sin\frac{n-2}{2}\pi\, (a^2-b^2)^{\frac{2-n}{2}}\, (a^2-r^2)^{-1}\, (b^2-r^2)^{\frac{n-2}{2}},$$

there will be equilibrium of the fluid within the inner sphere.

The value of $\rho$ is evidently negative if $n$ is less than 2.

Green then determines, from this value of the density, the whole quantity of fluid within the sphere whose radius is $b$, and then by equating this to $-E$, the whole quantity of negative fluid, he determines the radius, $b$, of the inner sphere, so that it shall just contain the whole of the negative fluid.

H. Cavendish's (unpublished) account of his determination of the Law of electric force.
(Ref. 2 pp. 104-113 and pp. 254-255 and pp 282-283)

# EXPERIMENTS ON ELECTRICITY.

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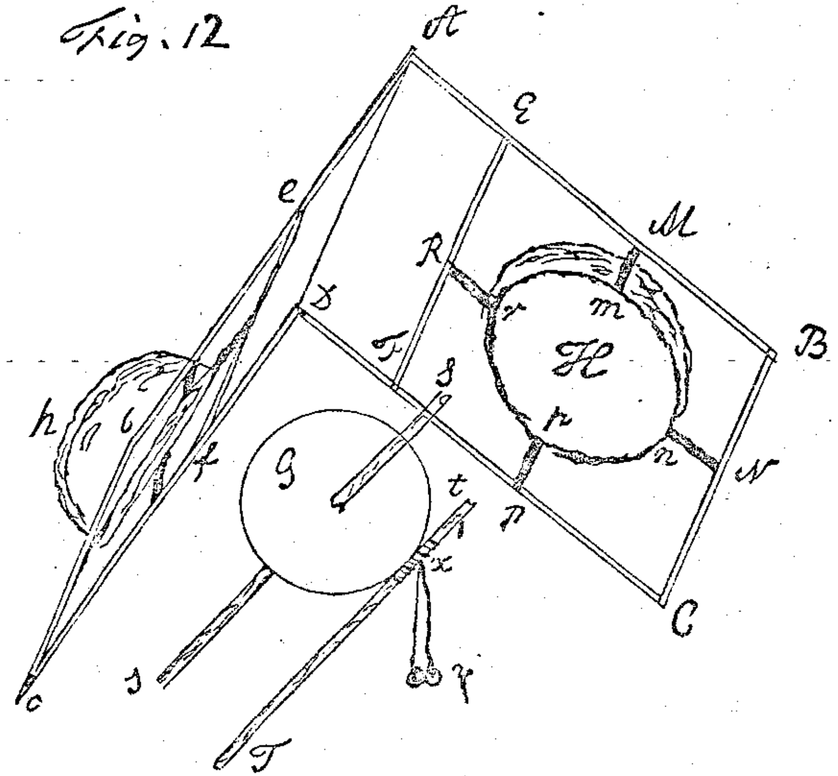
## EXPERIMENTAL DETERMINATION OF THE LAW OF ELECTRIC FORCE.

217] I now proceed to give an account of the experiments, in all of which I shall suppose, according to the received opinion, that the electricity of glass is positive, but it is not at all material to the purpose of this paper whether it is so or not, for if it was negative, all the experiments would agree equally well with the theory.

218] EXPERIMENT I. The intention of the following experiment was to find out whether, when a hollow globe is electrified,

Fig. 12






a smaller globe inclosed within it and communicating with the outer one by some conducting substance is rendered at all over or undercharged; and thereby to discover the law of the electric attraction and repulsion.

219] I took a globe 12·1 inches in diameter, and suspended it by a solid stick of glass run through the middle of it as an axis, and covered with sealing-wax to make it a more perfect non-conductor of electricity. I then inclosed this globe between two hollow pasteboard hemispheres, 13·3 inches in diameter, and about $\frac{1}{20}$ of an inch thick, in such manner that there could hardly be less than $\frac{4}{10}$ of an inch distance between the globe and the inner surface of the hemispheres in any part, the two hemispheres being applied to each other so as to form a complete sphere, and the edges made to fit as close as possible, notches being cut in each of them so as to form holes for the stick of glass to pass through.

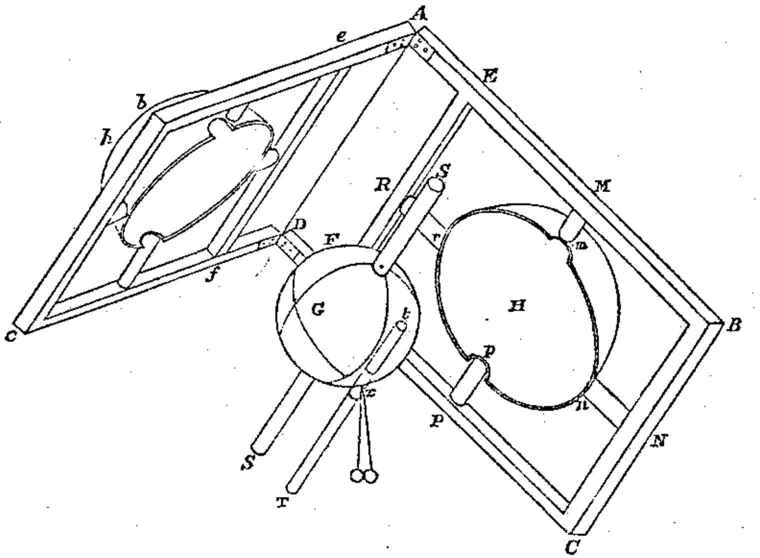
By this means I had an inner globe included within an hollow globe in such manner that there was no communication by which the electricity could pass from one to the other.

I then made a communication between them by a piece of wire run through one of the hemispheres and touching the inner globe, a piece of silk string being fastened to the end of the wire, by which I could draw it out at pleasure.

220] Having done this I electrified the hemispheres by means of a wire communicating with the positive side of a Leyden vial, and then, having withdrawn this wire, immediately drew out the wire which made a communication between the inner globe and the outer one, which, as it was drawn away by a silk string, could not discharge the electricity either of the globe or hemispheres. I then instantly separated the two hemispheres, taking care in doing it that they should not touch the inner globe, and applied a pair of small pith balls, suspended by fine linen threads, to the inner globe, to see whether it was at all over or undercharged.

221] For the more convenient performing this operation, I made use of the following apparatus. It is more complicated, indeed, than was necessary, but as the experiment was of great importance to my purpose, I was willing to try it in the most accurate manner.

Fig. 12.




*ABCDEF* and *AbcDef* (Fig. 12) are two frames of wood of the same size and shape, supported by hinges at *A* and *D* in such manner that each frame is moveable on the horizontal line *AD* as an axis. *H* is one of the hemispheres, fastened to the frame *ABCD* by the four sticks of glass *Mm*, *Nn*, *Pp*, and *Rr*, covered with sealing-wax. *h* is the other hemisphere fastened in the same manner to the frame *AbcD*. *G* is the inner globe, suspended by the horizontal stick of glass *Ss*, the frame of wood by which *Ss* and the hinges at *A* and *D* are supported being not represented in the figure to avoid confusion.

*Tt* is a stick of glass with a slip of tinfoil bound round it at *x*, the place where it is intended to touch the globe, and the pith balls are suspended from the tinfoil.

The hemispheres were fixed within their frames in such manner that when the frames were brought near together the edges of the hemispheres touched each other all round as near as might be, so as to form a complete sphere, and so that the inner globe was inclosed within them without anywhere touching them, but on the contrary being at nearly the same distance from them in all parts.



222] It was also so contrived, by means of different strings, that the same motion of the hand which drew away the wire by which the hemispheres were electrified, immediately after that was done, drew out the wire which made the communication between the hemispheres and the inner globe, and immediately after that was drawn out, separated the hemispheres from each other and approached the stick of glass *Tt* to the inner globe. It was also contrived so that the electricity of the hemispheres and of the wire by which they were electrified was discharged as soon as they were separated from each other, as otherwise their repulsion might have made the pith balls to separate, though the inner globe was not at all overcharged.

The inner globe and hemispheres were also both coated with tinfoil to make them the more perfect conductors of electricity.

223] In trying the experiments a coated glass jar was connected to the wire by which the hemispheres were electrified, and this wire was withdrawn so as not to touch the hemispheres till the jar was sufficiently charged. It was then suffered to rest on them for two or three seconds and then withdrawn, and the hemispheres separated as above described.

224] An electrometer also was fastened to the prime conductor by which the coated jar was electrified, by which means the jar and consequently the hemispheres were always electrified in the same degree. This electrometer as well as the pith balls will be described in [Arts. 244 and 248]; the strength of the electricity was the same as was commonly used in the following experiments, and is described in [Arts. 263, 329, 359, 520].

225] My reason for using the glass jar was that without it it would have been difficult either to have known to what degree the hemispheres were electrified or to have kept the electricity of the same strength for a second or two together, and if the wire had been suffered to have rested on the hemispheres while the jar was charging, I was afraid that the electricity might have spread itself gradually on the sticks of glass which supported the globe and hemispheres, which might have made some error in the experiment.

226] From this manner of trying the experiment it appears: First, that at the time the hemispheres are electrified, there is a perfect communication by metal between them and the inner globe, so that the electricity has free liberty to enter the inner globe if it has any disposition to do so, and moreover that this communication is not taken away till after the wire by which the hemispheres are electrified is removed.

Secondly, before the hemispheres begin to be separated from each other, the wire which makes the communication between them and the globe is taken away, so that there is no longer any communication between them by any conducting substance.

Thirdly, from the manner in which the operation is performed, it is impossible for the hemispheres to touch the inner globe while they are removing, or even to come within $\frac{4}{10}$ ths of an inch of it.

And Fourthly, the whole time of performing the operation is so short, that no sensible quantity of electricity can escape from the inner globe, between the time of taking away the communication between that and the hemispheres, and the approaching the pith balls to it, so that the quantity of electricity in the globe when the pith balls are approached to it cannot be sensibly different from what it is when it is inclosed within the hemispheres and communicating with them.

227] The result was, that though the experiment was repeated several times*, I could never perceive the pith balls to separate or show any signs of electricity.

228] That I might perceive a more minute degree of electricity in the inner globe, I tried the experiment in a different manner, namely, before the hemispheres were electrified, I electrified the pith balls positively, making them separate about one inch. When the hemispheres were then separated, and the tinfoil, $x$, brought in contact with the globe, and consequently the electricity of the pith balls communicated to the globe, they still continued to separate, though but just sensibly. I then repeated the experiment in the same manner, except that the pith balls were negatively electrified in the same degree that they before were positively. They still separated negatively after being brought in contact with the globe, and in the same degree that they before did positively.

[* Dec. 18—24, 1772, Arts. 512, 513, and April 4, 1773, Art. 562.]



229] It must be observed that if the globe was at all overcharged the pith balls should separate further when they were previously positively electrified than when negatively, as in the first case the pith balls must evidently separate further than they would do if the globe was not overcharged, and in the latter case less.

Moreover, a much smaller degree of electricity may be perceived in the globe by this manner of trying the experiment than the former, for when the pith balls have already got a sufficient quantity of electricity in them to make them separate, a sensible difference will be produced in their degree of divergence by the addition of a quantity of fluid several times less than what was necessary to make them separate at first. It is plain that this method of trying the experiment is not just, unless the hemispheres are electrified in nearly the same degree when the pith balls are previously electrified positively as when negatively, which was provided for by the electrometer.

230] In order to find how small a quantity of electricity in the inner globe might have been discovered by this experiment, I took away the hemispheres with their frames, leaving the globe and the pith balls as before. I then took a piece of glass, coated as a Leyden vial, which I knew by experiment contained not more than $\frac{1}{50}$ th of the quantity of redundant fluid on its positive side that the jar by which the hemispheres were electrified did, when both were charged from the same conductor.

I then electrified this coated plate to the same degree, as shewn by the electrometer, that the jar was in the former experiment, and then separated it from the prime conductor, and communicated its electricity to the jar, which was not at all electrified. Consequently the jar contained only $\frac{1}{60}$ th part of the redundant fluid in this experiment that it did in the former, for the coated plate and jar together contained only $\frac{1}{50}$ th, and therefore the jar alone contained only $\frac{1}{60}$ th.

By means of this jar, thus electrified, I electrified the globe in the same manner that the hemispheres were in the former experiment, and immediately after the electrifying wire was withdrawn, approached the pith balls. The result was that by previously electrifying the balls, as in the second way of trying the experiment, the electricity of the globe was very manifest, as the balls separated very sensibly more when they were previously electrified positively than when negatively, but the electricity of the globe was not sufficient to make the balls separate, unless they were previously electrified.

It is plain that the quantity of redundant fluid communicated to the globe in this experiment was less than $\frac{1}{60}$th part of that communicated to the hemispheres in the former experiment, for if the hemispheres themselves had been electrified they would have received only $\frac{1}{60}$th of the redundant fluid they did before, and the globe, as being less, received still less electricity.

231] It appears, therefore, that if a globe 12·1 inches in diameter is inclosed within a hollow globe 13·3 inches in diameter, and communicates with it by some conducting substance, and the whole is positively electrified, the quantity of redundant fluid lodged in the inner globe is certainly less than $\frac{1}{60}$th of that lodged in the outer globe, and that there is no reason to think from any circumstance of the experiment that the inner globe is at all overcharged.

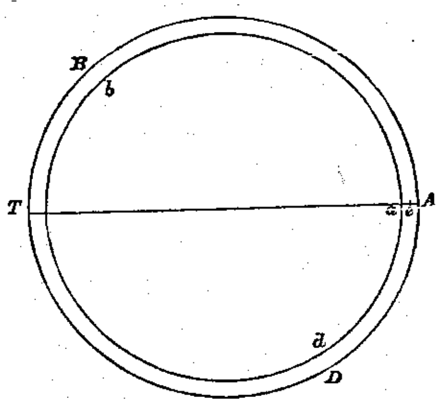
232] Hence it follows that the electric attraction and repulsion must be inversely as the square of the distance, and that when a globe is positively electrified, the redundant fluid in it is lodged intirely on its surface.

For by Prop. V. [Art. 20], if it is according to this law, the whole redundant fluid ought to be lodged on the outer surface of the hemispheres, and the inner globe ought not to be at all over or undercharged, whereas, if it is inversely as some higher power of the distance than the square, the inner globe ought to be in some degree overcharged.

233] For let $ADB$ (Fig. 13) be the hemispheres and $adb$ the inner globe, and $Aa$ the wire by which a communication is made between them. By Lemma IV. [Art. 18], if the electric attraction and repulsion is inversely as some higher power of the distance than the square, the redundant fluid in $ABD$ repels a particle of fluid placed anywhere in the wire $Aa$ towards the center, and consequently, unless the inner globe was sufficiently overcharged to prevent it, some fluid would flow from the hemispheres to the globe.



Fig. 13.




But if the electric attraction and repulsion is inversely as some lower power of the distance than the square, the redundant fluid in $ABD$ impels the particle in the contrary direction, that is, from the center, and therefore the inner globe must be undercharged.

234] In order to form some estimate how much the law of the electric attraction and repulsion may differ from that of the inverse duplicate ratio of the distances without its having been perceived in this experiment, let $AT$ be a diameter of the two concentric spheres $ABD$ and $abd$, and let $Aa$ be bisected in $e$. $Ae$ in this experiment was about ·35 of an inch and $Te$ 13·1 inches, therefore if the electric attraction and repulsion is inversely as the $2+\frac{1}{50}$th power of the distance, it may be shewn that the force with which the redundant fluid in $ABD$ repels a particle at $e$ towards the center is to that with which the same quantity of fluid collected in the center would repel it in the contrary direction as 1 to 57.

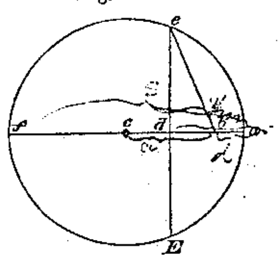
But as the law of repulsion differs so little from the inverse duplicate ratio, the redundant fluid in the inner globe will repel the point $e$ with very nearly the same force as if it was all collected in the center, and therefore if the redundant fluid in the inner globe is $\frac{1}{57}$th part of that in $ABD$ the particle at $e$ will be in equilibrio, and as $e$ is placed in the middle between $A$ and $a$, there is the utmost reason to think that the fluid in the whole wire $Aa$ will be so too. We may therefore conclude that the electric attraction and repulsion must be inversely as some power of the distance between that of the $2+\frac{1}{50}$th and that of the $2-\frac{1}{50}$th, and there is no reason to think that it differs at all from the inverse duplicate ratio*.

235] EXPERIMENT II. A similar experiment was tried with a piece of wood 12 inches square and 2 inches thick, inclosed between two wooden drawers each 14 inches square and 2 inches deep on the outside, so as to form together a hollow box 14 inches square and 4 thick, the wood of which it was composed being ·5 to ·3 of an inch thick.

The experiment was tried in just the same manner as the former. I could not perceive the inner box to be at all over or undercharged, which is a confirmation of what was supposed at the end of Prop. IX. [Art. 41]—that when a body of any shape is overcharged, the redundant fluid is lodged entirely on the surface, supposing the electric attraction and repulsion to be inversely as the square of the distance†.

## DEMONSTRATION OF COMPUTATIONS IN [ART. 234].

Fig. 13 *a*.




Let $aef$ be a sphere, $c$ its center, $b$ any point within it, $af$ a diameter, $Ee$ any plane perpendicular to $af$.

Let $cb=a$, $ba=d$, $bf=s$ and $ad=x$, and let the repulsion be inversely as the $n$ power of the distance. The convex surface of the segment $Eae$ is to that of the whole globe as $ad : af$, and therefore if the point $d$ is supposed to flow towards $f$, the fluxion of the surface $Eae$ is proportional to $\dot{x}$, and the fluxion of its repulsion on $b$ in the direction $dc$ is proportional to

$$\frac{\dot{x}(d-x)}{be^{n+1}},$$

or may be represented thereby, but

$$be^2=(d-x)^2+x(2a+2d-x)=d^2+2ax,$$

therefore the fluxion of the repulsion is

$$\frac{\dot{x}(d-x)}{(d^2+2ax)^{\frac{n+1}{2}}},$$

* [Note 19.] † [Art. 561.]



the variable part of the fluent of which is

$$\frac{-2ad-d^2}{4a^2\frac{n-1}{2}(d^2+2ax)^{\frac{n-1}{2}}}-\frac{(d^2+2ax)^{\frac{3-n}{2}}}{4a^2\frac{3-n}{2}};$$

but when $x$ is nothing, $d^2+2ax$, or $be^2=d^2$, and when $x=af$, or $s+d$, it $=s^2$, therefore the whole fluent generated while $b$ moves from $a$ to $f$ is

$$\frac{2ad+d^2}{2a^2(n-1)}\left(\frac{1}{d^{n-1}}-\frac{1}{s^{n-1}}\right)+\frac{d^{3-n}-s^{3-n}}{2a^2(3-n)};$$

but the repulsion of all the fluid collected in the center on $b$

$$=\frac{s+d}{a^n},$$

and $$a=\frac{s-d}{2},$$

and $$2ad+d^2=ds,$$

therefore the repulsion of the surface of the globe is to that of the same quantity of fluid collected in the center as

$$\frac{ds}{n-1}\times\frac{s^{n-1}-d^{n-1}}{(ds)^{n-1}}+\frac{d^{3-n}-s^{3-n}}{3-n}:\frac{2(s+d)}{a^{n-2}},$$

or as $$\frac{s^{n-1}-d^{n-1}}{(n-1)(ds)^{n-2}}+\frac{d^{3-n}-s^{3-n}}{3-n}:\frac{(s+d)\,2^{n-1}}{(s-d)^{n-2}},$$

or dividing by $s^{3-n}$, as

$$\frac{s^{n-2}}{d^{n-2}(n-1)}-\frac{d}{s(n-1)}+\frac{d^{3-n}}{s^{3-n}(3-n)}-\frac{1}{3-n}:\frac{s+d}{s}\left(\frac{s-d}{s}\right)^{2-n}2^{n-1},$$

or as $$\frac{p^{2-n}-p}{n-1}+\frac{p^{3-n}-1}{3-n}:(1+p)\,2^{n-1}(1-p)^{2-n},$$

supposing $\frac{d}{s}$ to be called $p$.

T.C. Maxwell: Improved version of, and theory of the Cavendish Experiment.
Ref. 2 pp 415-422

## NOTE 19, ART. 234.

*Cavendish's Experiment on the Charge of a Globe between two Hemispheres.*

This experiment has recently been repeated at the Cavendish Laboratory in a somewhat different manner.

The hemispheres were fixed on an insulating stand, so as to form a spherical shell concentric with the globe, which stood inside the shell upon a short piece of a wide ebonite tube.

By this arrangement, since during the whole experiment the potentials of the globe and sphere remained sensibly equal, the insulating support of the globe was never exposed to the action of any sensible electromotive force, and therefore had no tendency to become charged.

If the other end of the insulator supporting the globe had been connected to earth, then, when the potential of the globe was high, electricity would have crept from it along the insulator, and would have crept back again when, in the second part of the experiment, the potential of the globe was sensibly zero. In fact this was the chief source of disturbance in Cavendish's experiment. See Art. 512.

Instead of removing the hemispheres before testing the potential of the globe, they were left in their position, but discharged to earth. The effect on the electrometer of a given charge of the globe was less than if the hemispheres had been removed, but this disadvantage was more than compensated by the perfect security from all external electric disturbances afforded by the conducting shell.

The short wire which formed the communication between the shell and the globe was fastened to a small metal disk hinged to the shell, and acting as a lid to a small hole in it, so that when the lid and its wire were lifted up by means of a silk string, the electrode of the electrometer could be made to dip into the hole in the shell and rest on the globe within.

The electrometer was Thomson's Quadrant Electrometer.

The case of the electrometer, and one of the electrodes, were permanently connected to earth, and the testing electrode was also kept connected to earth, except when used to test the potential of the globe.

To estimate the original charge of the shell, a small brass ball was placed on an insulating stand at a distance of about 60 cm. from the centre of the shell.

The operations were conducted as follows:—

The lid was closed, so that the shell communicated with the globe by the short wire.

A Leyden jar was charged from a machine in another room, the shell was charged from the jar, and the jar was taken out of the room again.

The small brass ball was then connected to earth for an instant, so as to give it a negative charge by induction, and was then left insulated.

The lid was then lifted up by means of the silk string, so as to take away the communication between the shell and the globe.

The shell was then discharged and kept connected to earth.

The testing electrode of the electrometer was then disconnected from earth, and made to pass through the hole in the shell so as to touch the globe within without touching the shell.

Not the slightest deflexion of the electrometer could be observed.

To test the sensitiveness of the apparatus, the shell was disconnected from earth and connected to the electrometer. The small brass ball was then discharged to earth.

This produced a large positive deflexion of the electrometer.

Now in the first part of the experiment, when the brass ball was connected to earth, it became charged negatively, the charge being about $\frac{1}{54}$ of the original positive charge of the shell.

When the shell was afterwards connected to earth the small ball induced on it a positive charge equal to about one-ninth of its own negative charge. When at the end of the experiment the small ball was discharged to earth, this charge remained on the shell, being about $\frac{1}{486}$ of its original charge.

Let us suppose that this produces a deflexion $D$ of the electrometer, and let $d$ be the largest deflexion which could escape observation in the first part of the experiment.

Then we know that the potential of the globe at the end of the first part of the experiment cannot differ from zero by more than

$$\pm \frac{1}{486} \frac{d}{D} V,$$

where $V$ is the potential of the shell when first charged.

But it appears from the mathematical theory that if the law of repulsion had been as $r^{-(2+q)}$, the potential of the globe when tested would have been by equation (25), p. 421,

$$0{\cdot}1478 \times qV.$$

Hence $q$ cannot differ from zero by more than $\pm \frac{1}{72} \frac{d}{D}$.

Now, even in a rough experiment, $D$ was certainly more than $300d$. In fact no sensible value of $d$ was ever observed. We may therefore conclude that $q$, the excess of the true index above 2, must either be zero, or must differ from zero by less than

$$\pm \tfrac{1}{21600}.$$

*Theory of the Experiment.*

Let the repulsion between two charges $e$ and $e'$ at a distance $r$ be

$$f = ee' \phi(r), \qquad (1)$$

where $\phi(r)$ denotes any function of the distance which vanishes at an infinite distance.

The potential at a distance $r$ from a charge $e$ is

$$V = e \int_r^\infty \phi(r)\, dr. \qquad (2)$$

Let us write this in the form

$$V = e \frac{1}{r} f'(r), \qquad (3)$$

where

$$f'(r) = \frac{df(r)}{dr}, \qquad (4)$$

and $f(r)$ is a function of $r$ equal to $\int r \left[ \int_r^\infty \phi(r)\, dr \right] dr$.

We have in the first place to find the potential at a given point $B$ due to a uniform spherical shell.

Let $A$ be the centre of the shell, $a$ its radius, $\alpha$ its whole charge, and $\sigma$ its surface-density, then

$$\alpha = 4\pi a^2 \sigma. \qquad (5)$$

Take $A$ for the centre of spherical co-ordinates and $AB$ for axis, and let $AB = b$.

Let $P$ be a point on the sphere whose spherical co-ordinates are $\theta$ and $\phi$, and let $BP = r$, then

$$r^2 = a^2 - 2ab \cos\theta + b^2. \qquad (6)$$

The charge of an element of the shell at $P$ is

$$\sigma a^2 \sin\theta\, d\theta\, d\phi = \frac{1}{4\pi} \alpha \sin\theta\, d\theta\, d\phi. \qquad (7)$$

The potential at $P$ due to this element is

$$\frac{1}{4\pi} \alpha \frac{f'(r)}{r} \sin\theta\, d\theta\, d\phi, \qquad (8)$$

and the potential due to the whole shell is therefore

$$V = \int_0^{2\pi} \int_0^\pi \frac{1}{4\pi} \alpha \frac{f'(r)}{r} \sin\theta\, d\theta\, d\phi. \qquad (9)$$

Integrating with respect to $\phi$ from 0 to $2\pi$,

$$V = \int_0^\pi \tfrac{1}{2} \alpha \frac{f'(r)}{r} \sin\theta\, d\theta. \qquad (10)$$

Differentiating (6) with respect to $\theta$,

$$r dr = ab \sin\theta\, d\theta. \qquad (11)$$

Hence,

$$V = \int_{r_1}^{r_2} \frac{1}{2} \frac{\alpha}{ab} f'(r)\, dr = \frac{1}{2} \frac{\alpha}{ab} \{f(r_2) - f(r_1)\}, \qquad (12)$$

the upper limit $r_2$ being always $a + b$, and the lower limit $r_1$ being $a - b$ when $a > b$, and $b - a$ when $a < b$.

Hence, for a point inside the shell

$$V = \frac{\alpha}{2ab} \{f(a+b) - f(a-b)\}, \qquad (13)$$

for a point on the shell itself

$$V = \frac{\alpha}{2a^2} f(2a), \qquad (14)$$

and for a point outside the shell

$$V = \frac{\alpha}{2ab} \{f(b+a) - f(b-a)\}. \qquad (15)$$

We have next to determine the potentials of two concentric spherical shells, the radius of the outer shell being $a$ and its charge $\alpha$, and that of the inner shell being $b$ and its charge $\beta$.

Calling the potential of the outer shell $A$, and that of the inner $B$, we find by what precedes,

$$A = \frac{\alpha}{2a^2} f(2a) + \frac{\beta}{2ab}\{f(a+b) - f(a-b)\}, \qquad (16)$$

$$B = \frac{\beta}{2b^2} f(2b) + \frac{\alpha}{2ab}\{f(a+b) - f(a-b)\}. \qquad (17)$$

In the first part of the experiment the shells communicate by the short wire and are both raised to the same potential, say $V$.

Putting $A = B = V$ and solving equations (16), (17), we find for the charge of the inner shell

$$\beta = 2Vb\frac{bf(2a) - a\{f(a+b) - f(a-b)\}}{f(2a)f(2b) - \{f(a+b) - f(a-b)\}^2}. \qquad (18)$$

In the original experiment of Cavendish the hemispheres forming the outer shell were removed altogether from the globe and discharged. The potential of the inner shell or globe would then be

$$B_1 = \frac{\beta}{2b^2} f(2b). \qquad (19)$$

In the form of the experiment as repeated at the Cavendish Laboratory, the outer shell was left in its place, but was connected to earth, so that $A = 0$. In this case we find for the potential of the inner shell when tested by the electrometer

$$B_2 = V\left\{1 - \frac{a}{b}\frac{f(a+b) - f(a-b)}{f(2a)}\right\}. \qquad (20)$$

Let us now assume with Cavendish, that the law of force is some inverse power of the distance, not differing much from the inverse square, that is to say, let

$$\phi(r) = r^{-(2+q)}, \qquad (21)$$

then

$$f(r) = \frac{1}{1-q^2} r^{1-q}. \qquad (22)$$

If we suppose $q$ to be a small numerical quantity, we may expand $f(r)$ by the exponential theorem in the form

$$f(r) = \frac{1}{1-q^2} r\{1 - q\log r + \frac{1}{1\,.\,2}(q\log r)^2 - \&c.\}, \qquad (23)$$

and if we neglect terms involving $q^2$, equations (19) and (20) become

$$B_1 = \tfrac{1}{2}\frac{a}{a-b} Vq\left[\frac{a}{b}\log\frac{a+b}{a-b} - \log\frac{4a^2}{a^2-b^2}\right], \qquad (24)$$

$$B_2 = \tfrac{1}{2} Vq\left[\frac{a}{b}\log\frac{a+b}{a-b} - \log\frac{4a^2}{a^2-b^2}\right]. \qquad (25)$$

Laplace [Mec. Cel. I. 2] gave the first direct demonstration that no function of the distance except the inverse square can satisfy the condition that a uniform spherical shell exerts no force on a particle within it.

If we suppose that $\beta$, the charge of the inner sphere, is always accurately zero, or, what comes to the same thing, if we suppose $B_1$ or $B_2$ to be zero, then

$$bf(2a) - af(a+b) - af(a-b) = 0.$$

Differentiating twice with respect to $b$, $a$ being constant, and dividing by $a$, we find

$$f''(a+b) = f''(a-b),$$

or, if $a - b = c$,

$$f''(c+2b) = f''(c),$$

which can be true only if

$$f''(r) = C_0 \text{ a constant.}$$

Hence,

$$f'(r) = C_0 r + C_1,$$

and

$$\int_r^\infty \phi(r)\,dr = \frac{1}{r}f'(r) = C_0 + \frac{1}{r}C_1,$$

whence,

$$\phi(r) = C_1\frac{1}{r^2}.$$

We may notice, however, that though the assumption of Cavendish, that the force varies as some inverse power of the distance, appears less general than that of Laplace, who supposes it to be any function of the distance, it is the most general assumption which makes the ratio of the force at two different distances a function of the ratio of these distances.

If the law of force is not a power of the distance, the ratio of the forces at two different distances is not a function of the ratio of the distances alone, but also of one or more linear parameters, the values of which if determined by experiment would be absolute physical constants, such as might be employed to give us an invariable standard of length.

Now although absolute physical constants occur in relation to all the properties of matter, it does not seem likely that we should be able to deduce a linear constant from the properties of anything so little like ordinary matter as electricity appears to be.

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# A Very Accurate Test of Coulomb's Law of Force Between Charges

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The exponent 2 in Coulomb's inverse square law of force between charges in empty space has been found experimentally to be correct to within 1 part in $10^9$. The well-known electrostatic experiment of Cavendish and Maxwell with concentric metal globes was replaced by a quasi-static method in which the difficulties due to spontaneous ionization and contact potentials were avoided. A "resonance electrometer" (undamped galvanometer with amplifier) was placed within the globes, the input resistor of the amplifier forming a permanent link connecting them, so as to measure any variable potential difference between them. It was shown theoretically that the presence of the resonance electrometer would have no effect on the result and that it could replace electrically a part of the inner globe. The galvanometer was observed through a "conducting window" at the top, made so by covering it with salt water. No effect was observed when a harmonically alternating high potential $V$ (>3000 volts), from a specially designed "condenser generator" operating at the low resonance frequency of the galvanometer, was applied to the outer globe. The sensitivity was such that a voltage $v=10^{-6}$ volt was easily observable above the small fluctuations due to Brownian motion.

If the exponent in the law of force were not exactly 2 but rather $2\pm q$ then $q<v/VF(a, b)$ where $F(a, b)=0.169$, $a$ and $b$ being the radii of the globes. This gives $q<2\times 10^{-9}$ in space remote from matter. The formula for $F(a, b)$ was derived by Maxwell's theory in which the effect of gravity is assumed negligible. Reasons are given for believing that this assumption does not invalidate the result.

## Introduction

THE inverse square law of force between electric charges was tested by Coulomb with his torsion balance and more precisely by Maxwell using a modification of a method invented by Cavendish. Maxwell used a spherical air condenser consisting of two insulated spherical shells ("globes"), the outer having a small hole in it so that the inner could be tested for charge by inserting an electrode from a Thomson electrometer. This hole was closed by a small lid carrying an inward projection which simultaneously connected the two shells together. The outer shell was then charged to a high potential $V$, the lid and connector removed by a silk thread, and the outer shell earthed. Maxwell showed[1] that if the exponent in Coulomb's law is not 2 but $2\pm q$ the magnitude of the potential of the inner shell (neglecting higher orders of $q$) would then be $VqF(a, b)$, where $F(a, b)$ is a known function of the ratio of the radii $a$ and $b$ of the outer and inner spheres, namely:

$$F(a, b)=\frac{n}{2}\log\frac{n+1}{n-1}-\tfrac{1}{2}\log\frac{4n^2}{n^2-1},\quad n=\frac{a}{b}.$$

Since the potential remaining on the inner shell was found by the experiment to be less in magnitude than a small detectable potential $v$, then $q<v/VF(a, b)$. In this way Maxwell obtained the result still generally quoted in text-books, $q<1/21,600$.

Professor A. Wilmer Duff directed the attention of the writers to the problem of improving the accuracy of this test, i.e., of reducing this upper limit for the value of $q$, with the aid of the more sensitive electrometers now available.

## Preliminary Investigations

In the derivation of the expression $VqF(a, b)$ it is assumed that the globes are exact spheres and that the charges are distributed uniformly over them. These assumptions are made merely to facilitate the computation, and the order of magnitude of the result is unaffected if the globes are only approximately spherical and the distribution only approximately uniform. The smallness of the result for the upper limit for $q$ depends not upon a close approach to sphericity, but rather upon the great difference in the magnitudes of the applied voltage $V$ and the smallest detectable voltage $v$.

At first sight one would suppose that an enormous improvement in the accuracy of the test by Maxwell's method would be possible, due to recent advances in the production of high voltages and in the detection of extremely small voltages. Curiously enough, however, Maxwell apparently reached about the limit attainable by his method even with modern equipment. In the first place with what is now known about the disturbing effect of unavoidable *spontaneous ionization*[2] it is a question how long a charge on the inner globe, even as small as that involved in Maxwell's experiment, would remain unaffected. The most serious difficulty in going to great sensitivities in this method is due, however, to *contact potentials*. It is the experience of the writers that in the measurement of *an isolated charge* 0.001 volt is about the practical limit. This is entirely different from the use of contacts in the measurement of potentials associated with small currents, as in ionization measurements,

[1] Maxwell, *Electricity and Magnetism*, I, p. 83.

where the energies involved are greater and initial effects become smoothed over with time. The following statement of Hoffman[3] in reference to his electrometer is interesting in this connection: "The contact potential difference of the duants (entirely platinized) with respect to each other proved to be of the order of magnitude of a thousandth of a volt; the drop to the needle is greater."

The possibility of applying the Maxwell method with a vacuum between the globes and no contacts was studied, the potential of the inner globe being measured by its effect on a small inductor which was connected to an amplifier and moved in the vicinity of the charge. Even here there seemed to exist somewhat variable space gradients of potential of the order of 0.0001 volt.

## Detector Inside Globe

In the method finally adopted the effects of contacts were entirely eliminated by placing the detector inside the inner globe, and connecting it permanently so as to indicate any change in the potential of the inner globe relative to the outer one. This modification of Maxwell's method, in particular the presence of the conducting mass of the detector within the inner globe, changes somewhat the conditions under which the inequality $q<v/VF(a, b)$ was derived. In order to make sure that this formula would still be applicable we extended the theory as given by Maxwell to the case of three concentric spherical shells of radii $a$, $b$, $c$ (where $a>b>c$), the two inner ones being connected together but insulated from the outer one. It was thus found that charging the outer shell to the potential $V$ would cause a potential difference between it and the inner shells given by the same expression $VqF(a, b)$ if the exponent is $2\pm q$, where $F(a, b)$ is the function already stated. If this potential difference is shown experimentally to be less than $v$ we have as before

$$q<v/VF(a, b).$$

Calculation also showed that the magnitude of the charge necessary to neutralize this potential difference is found by multiplying it by the capacity $ab/(a-b)$, whether or not the innermost shell is present.

Since, then, the presence of a conducting body inside the inner globe and connected to it has no effect on the results, it was possible to arrange dimensions so that the detector, which consisted of a tube electrometer and amplifier, together with its shield was approximately equivalent to the lower half of the inner globe. This lower half could then be replaced by the detector.

[2] See, e.g., J. A. Bearden, Rev. Sci. Inst. 4, 271 (1933).
[3] G. Hoffman, Ann. d. Physik 52, 701 (1917).

## Construction of Globes and Detector

To serve as the outer globe A (Fig. 1) two approximately hemispherical shells, five feet in diameter, were constructed of sheet iron gores soldered together. One of these was mounted on a cylindrical porcelain insulator to form the lower half of A. The copper boxes containing the detector D were properly located in it, supported by porcelain insulators on a slat floor. The detector D consisted of a five-stage, resistance-capacity coupled amplifier designed for a frequency of about 2 cycles per second so as to operate a panel galvanometer G having this frequency and low damping. The first tube was an FP 54 with an input grid resistor of $10^{10}$ ohms. This was followed by three stages using PJ 11 tubes, which were enclosed in cast iron boxes suspended on rubber to avoid microphonic effects. The final stage of the amplifier was of conventional design. The sensitivity was adjusted so that the Johnson effect (Brownian heat motion) in the input resistor caused an average motion of the galvanometer of about one small division, corresponding to about $\frac{1}{2}$ microvolt.

The inner hemispherical dome B of four feet diameter, constructed of sheet iron gores similar to those of A, was mounted on Pyrex glass insulators and connected to the amplifier. Tests were made with the outer shell A connected to the grid of the first tube, the amplifier shield being connected to the dome B, and also with the grid connected to B and the amplifier shield connected to A. In neither case could any change in the potential of B relative to A be detected when A was properly charged and discharged.

The galvanometer G was illuminated by a lamp drawing current from the amplifier batteries, and after the upper half of A was in place it could be viewed through holes at the top of A and B. It was important that there be no break in the conducting surface of the outer globe A. The problem of a *conducting window* was solved by using a glass-bottomed vessel threaded into the hole in A, which contained a solution of ordinary salt in water with its surface flush with the outer surface of A. Submerged in the solution was a disk of fine wire gauze, covering the glass and soldered to the threaded rim of the vessel. The presence of the salt solution was essential to the success of the experiment.

## Quasi-Static Method

When the outer globe was charged and discharged simply by opening and closing circuits, the galvanometer gave deflections which were due to electromagnetic effects ($Mdi/dt=Md^2Q/dt^2$ where $Q$ is the charge on A and $M$ is the mutual conductance between A and B). It was necessary to devise a method for changing the charge $Q$ on

TEST OF COULOMB'S LAW

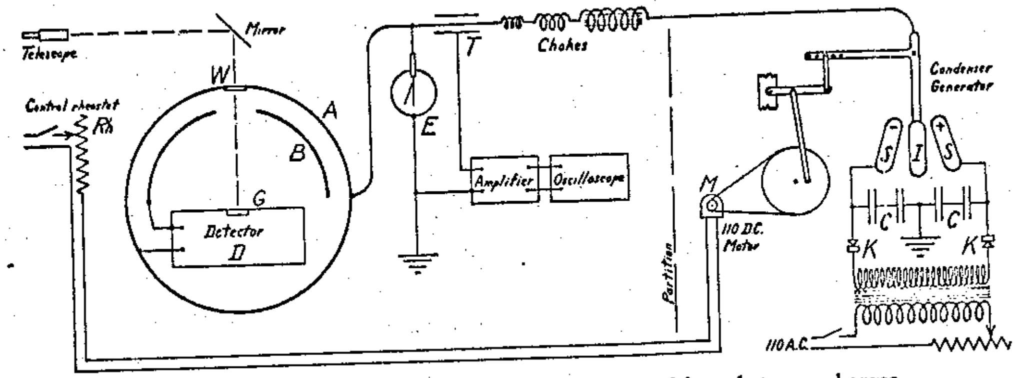



FIG. 1. Apparatus for testing the inverse square law of force between charges.

A in such a manner that these electromagnetic effects would never be large enough to be detectable. In other words, $di/dt$ had to be kept small. For this purpose the use of a resistor alone was not satisfactory, on account of the large value of $di/dt$ during a very short time at the beginning of the charge or discharge, and it was not feasible to construct a sufficiently large choke. A quasi-static method was therefore adopted.

In this method the detector was employed as a *resonance electrometer*. The high voltage was applied to the outer globe as a smooth sinusoidal wave of very low frequency, so as to build up a resonant vibration of the galvanometer. This had great advantages apart from that of sensitivity. The chief advantage may be explained as follows. The sensitivity of the amplifier is limited by Brownian heat fluctuations. These consist of all frequencies with equal energies, according to the equipartition law. The galvanometer responds in a resonant manner to the components among these with frequencies approximately equal to its own. The disturbance to be measured (signal) is at a relative disadvantage (by a factor depending on the damping of the galvanometer) as compared with this heat motion unless it too has a frequency equal to that of the galvanometer.

As indicated above, a low frequency was necessary to reduce electromagnetic effects. Calculation showed that with a frequency of about 2 cycles per second the inductive effect ($Mdi/dt$) would be entirely submerged in the fluctuations (about $\frac{1}{2}$ microvolt) due to heat motion in the detector.

### CONDENSER GENERATOR

There appears to be no commercially available source of such low frequency high voltage with smooth wave form. Transformers and filters are not practical at such low frequencies, nor is high voltage directly from an ordinary generator. What might be called a *condenser generator* was developed for the purpose. It may be described as consisting of three large metal combs: two stationary (stators) nearly parallel to each other with their teeth upward, and the third (the inductor) held with teeth downward and swung harmonically in and out of mesh with the other two alternately. It is shown diagrammatically at the right of Fig. 1 and disassembled in Fig. 2.

The maximum voltage provided by this generator is determined by the spark-over potential between the inductor I and the stators SS. No attempt was made to use extremely high voltages because of the disturbing effect of the radiation associated with brush discharge. The peculiar design of the generator was the result of an attempt to realize the two following conditions: a smooth sinusoidal wave form and a rapid increase of the gap between combs as the inductor moved away from either stator, so that the rising potential difference would not cause spark-over at any point.

The stators SS (Fig. 1) were charged by a transformer from the lighting circuit with the aid of the kenetrons KK. Their capacity to ground, which was increased by adding mica condensers CC in parallel, was large compared with that of the globe A, so that their potentials were not affected appreciably by the variation of their inductive relation with A as the inductor I swung in and out of mesh. Since these condensers retained their charges for hours, this arrangement also made it possible to turn off the kenetron charging system during the experiment, thus avoiding the difficulty of filtering out disturbances from this system.

## Arrangement of Apparatus

From Fig. 1 it will be seen that the generator was connected to the globe A through graduated chokes as a further protection against high frequencies. The applied voltage could be read directly on the Braun electrometer E for any position of the inductor I. For observing the wave form a cathode-ray oscilloscope was also coupled to the line through a tubular air condenser T. The amplifier shown in front of this oscilloscope was required on account of the high impedance of the condenser. The oscilloscope was provided with a 60-cycle horizontal sweep, which became a very slender ellipse owing to pick-up from the surroundings. This ellipse moved up and down with the applied voltage, retaining its smooth shape and thus proving the absence of high frequency disturbances.

If the variation of the voltage applied to the globe A produced an appreciable potential difference between A and the dome B, it would be indicated by resonant motion of the galvanometer G in the detector D connected between A and B as already described. G was observed through the conducting window W by means of a mirror and telescope as shown. The observer at the telescope could start and stop the motor M which drove the generator and, by means of the rheostat Rh and meters not shown, he could control the speed of M and thus keep the frequency of the applied voltage at the resonance value (130 per minute). This frequency adjustment could be checked at any time by blowing enough salt water out of the window W so that the galvanometer would respond.

## Method of Calibration

The connections for the calibration are shown in Fig. 3. The outer shell A was grounded, and a small known fraction $r/(R+r)$ of the voltage from the condenser generator was applied to the dome B by means of the high resistance potentiometer R+r, the frequency being kept at the resonance value. The amplitude of the voltage from the generator was read directly on the oscilloscope O (without sweep) which, being independent of frequency, could be calibrated by applying voltages from a battery as indicated. In this way a very nearly linear calibration for the resonance electrometer D was obtained.

In determining the smallest voltage $v$ detectable by D it was desirable to keep both the relaxation time and the impedance of the input of the amplifier as nearly as possible the same as under working conditions. The presence of the resistance r reduced slightly the fluctuations of the galvanometer due to heat motion. These were restored when a small air condenser c (with shield not shown) was inserted in the lead to the dome. By making allowance for the known impedance of this condenser the calibration was found to be as before. The minimum voltage $v$ observable above the usual heat fluctuation was thus determined with the calibrating device so loosely coupled that it had practically no effect on the sensitivity of the resonance electrometer.

## Result

With the apparatus operating properly at the resonance frequency, many trials with the amplitude of the applied voltage $V$ never less than 3000 volts and $v$ remaining very consistently equal to 1 microvolt convinced various observers that no change in the small heat motion of the galvanometer could be detected when the generator was started and stopped at random. For our apparatus $a=2.5$ ft., $b=2.0$ ft., and $F(a, b)=0.169$. Substitution in the inequality $q<v/VF(a, b)$ then yields $q<10^{-6}/3000\times0.169$, whence

$$q<2\times10^{-9}.$$

The exponent 2 in the inverse square law of force

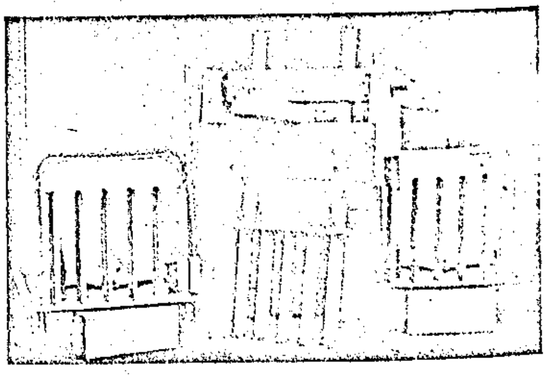


Fig. 2. Condenser generator disassembled.

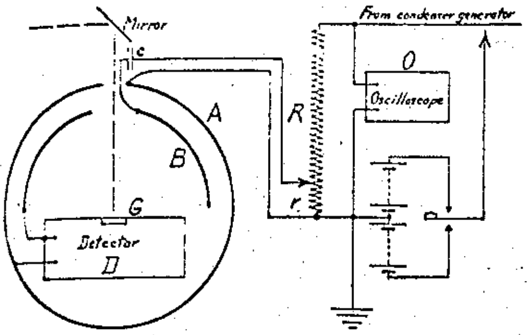



FIG. 3. Method of calibration.

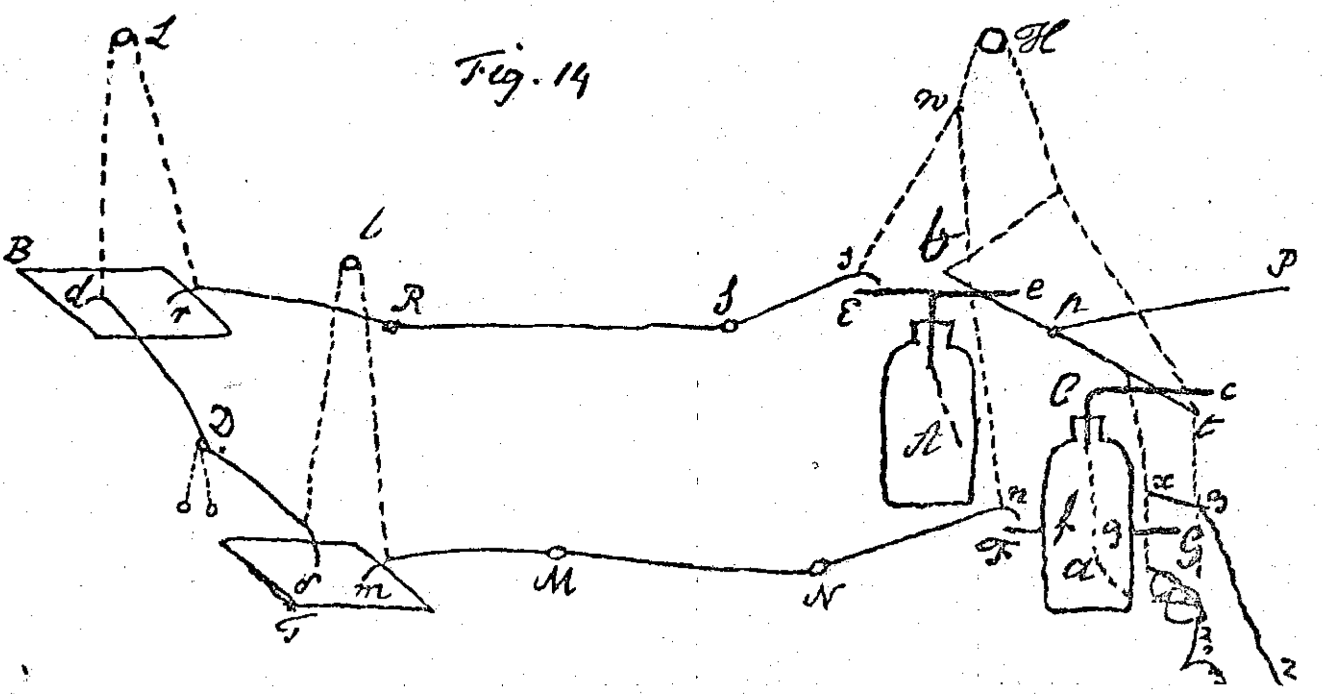
between charges in space remote from matter is thus shown to be correct to within one part in $10^9$.

### EFFECT OF GRAVITY

In view of the accuracy of the test some statements as to the possible effect of gravity are desirable. The expression for $F(a, b)$ was derived by means of classical electrodynamics in which the effect of gravity is neglected. It was assumed, for instance, that the charge density on the surface of a spherical conductor is the same everywhere. If electrons (inertial mass $m_\epsilon$, charge $-\epsilon$) have weight $m_\epsilon g$ the electron density on the conductor must be unsymmetrical, being greater near the bottom, in such a way that at any point of its surface the force of gravity on an electron is balanced by the electrical force due to the vertical potential gradient $dV/dh$, that is, $\epsilon dV/dh = m_\epsilon g$. This gives by integration a maximum potential difference over globe A of not more than $10^{-10}$ volt. Since this is far less than the minimum detectable voltage $v = 10^{-6}$ volt, such an effect is too small to cast any doubt on the result. Moreover, the force due to gravity may be thought of as neutralized by the electrical force due to the unsymmetrical electron density before any charge is applied to the globe. The applied charge would then be distributed symmetrically. Looked at in this way, gravity should have no effect on the experiment however accurately performed. We can then say that what we have tested is the law of force between charges in space remote from matter, that is, not in a gravitational field.

An objection to these statements might be raised on the ground that it should not be assumed that there is no interrelation between gravitational and electrical fields, and that it is conceivable that the gravitational field might, by virtue of this interrelation, affect the electrical field in just such a way as to give no variable potential differences inside the globes even if $q$ for empty space were greater than $2 \times 10^{-9}$. The possibility of such a coincidence seems remote, to say the least, especially in view of the magnitudes involved. However, we have considered the shielding action of the globe from the point of view of the general theory of relativity and have arrived at a conclusion which is even more reassuring. The reasoning on which this conclusion is based involves considerable mathematics and is left to a later paper.

The authors wish to express their appreciation to Professor A. W. Duff at whose suggestion this investigation was undertaken, to Professor H. H. Newell for his stimulating interest and advice, and to Mr. R. F. Field of the General Radio Company for helpful discussions regarding the action of amplifiers.




Cavendish's Sketch for Apparatus for Electrical Measurement (of 'Capacity')

## New Experimental Test of Coulomb's Law: A Laboratory Upper Limit on the Photon Rest Mass

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A high-frequency test of Coulomb's law is described. The sensitivity of the experiment is given in terms of a finite photon rest mass using the Proca equations. The null result of our measurement expressed in the form of the photon rest mass squared is $\mu^2 = (1.04 \pm 1.2)\times 10^{-19}$ cm$^{-2}$. Expressed as a deviation from Coulomb's law of the form $1/r^{2+q}$, our experiment gives $q = (2.7 \pm 3.1)\times 10^{-16}$. This result extends the validity of Coulomb's law by two orders of magnitude.

The testing of Coulomb's law (Gauss's law) by means of a null experiment dates back to Cavendish (1773).[1] The now classical test of Plimpton and Lawton[2] was performed in 1936, and showed that any difference in the exponent from 2 was smaller than $1\times 10^{-9}$. Recently two other groups have extended the accuracy of that result by two[3,4] and four[5] orders of magnitude. The result reported here represents an extension in accuracy over that obtained by Plimpton and Lawton by six orders of magnitude.

The experiment described here is a "high-frequency" null test of Coulomb's law. We make use of the fact that a $1/r^2$ force law does not give rise to any electric field on the inside of a closed conductor. A conducting shell that is about $1\frac{1}{2}$ m in diameter is charged to 10 kV peak to peak with a 4-MHz sinusoidal voltage. Centered inside of this charged conducting shell is a smaller conducting shell. Any deviation from the $1/r^2$ force law is detected by measuring the line integral of the electric field between these two shells with a detection sensitivity of about $10^{-12}$ V peak to peak.

The results of the experiment can be expressed in terms of the Proca equations,[6,7] a relativistically invariant linear generalization of Maxwell's equations, which are appropriate to describe the experimental system when a finite rest mass is assumed. Proca's equations for a particle of spin 1 and mass $m_0$ are[8]

$$(\Box + \mu^2)A_\nu = (4\pi/c)J_\nu, \qquad (1)$$

where $\mu = m_0 c/\hbar$. In three-dimensional notation, Gauss's law becomes

$$\nabla\cdot\vec{E} = 4\pi\rho - \mu^2\varphi. \qquad (2)$$

To calculate the sensitivity of the system, consider an idealized geometry consisting of two concentric, conducting, spherical shells of radii $R_1$ and $R_2$ ($R_2 > R_1$) with an inductor across (in parallel with) this spherical capacitor. To the outer shell is applied a potential $V_0 e^{i\omega t}$. An iterative solution for the field between the spheres may easily be found. Forming a spherical Gaussian surface at radius $r$ between the two shells and then using the approximation $\varphi(r) = V_0 e^{i\omega t}$ for this interior region, the integral of Eq. (2) over the volume interior to the Gaussian surface becomes

$$\int[\nabla\cdot\vec{E} - 4\pi\rho + \mu^2 V_0 e^{i\omega t}]d^3x = 0. \qquad (3)$$

Therefore $\vec{E}(r)$ is given by

$$\vec{E}(r) = (qr^{-2} - \tfrac{1}{3}\mu^2 V_0 e^{i\omega t} r)\hat{r}, \tag{4}$$

where $q$ is the total charge on the inner shell.

A complete solution of the fields inside a symmetrically charged single sphere of radius $R_2$ gives[9,10]

$$\vec{E}(r) = \frac{\mu^2}{k^2 r^2}\frac{R_2 V_0 e^{i\omega t}}{e^{-ikR_2} - e^{ikR_2}} \times [ikr(e^{-ikr} + e^{ikr}) - (e^{-ikr} - e^{ikr})]\hat{r} \tag{5}$$

and also $\vec{H} = 0$. Here $k^2 = \omega^2/c^2 - \mu^2$. A power series expansion of $\vec{E}(r)$ where $kr < 1$ [near zone] and $\omega/c > \mu$ gives

$$\vec{E}(r) = -\tfrac{1}{3}\mu^2 V_0 e^{i\omega t} r(1 - \tfrac{1}{10}k^2 r^2 + \tfrac{1}{6}k^2 R_2^2 \cdots)\hat{r}. \tag{6}$$

On neglecting the second-order term, which for this experiment produces an error of less than 1%, this equation reduces to a nonzero rest mass term which is the same as was derived rather simply above.

Since $\partial\vec{H}/\partial t$ is zero inside, $\oint \vec{E}\cdot dl = 0$. The voltage appearing across the inductor is then simply given by

$$\int_{R_1}^{R_2} \vec{E}\cdot d\vec{l} = \frac{q}{C} - \mu^2 \frac{V_0 e^{i\omega t}}{6}(R_2^2 - R_1^2). \tag{7}$$

The differential equation which describes this $LC$ circuit in the case of a nonzero rest mass is then

$$L\frac{d^2 q}{dt^2} + r\frac{dq}{dt} + \frac{q}{C} = \mu^2 \frac{[V_0 e^{i\omega t}]}{6}(R_2^2 - R_1^2). \tag{8}$$

The signal-to-noise ratio of a system described by this differential equation can be analyzed using conventional circuit theory. On doing this it turns out that the use of a high frequency, high-$Q$ circuits, a large apparatus, and as high as possible applied voltage $V_0$ serve to maximize the experimental sensitivity.

The experimental apparatus (Fig. 1) consists of five concentric icosahedrons. A 4-MHz rf voltage between the outer two shells (4 and 5) is obtained by pumping energy into the resonant circuit formed by the two shells and a high-$Q$ water cooled coil. A peak-to-peak voltage of 10 kV was achieved. A battery-powered lock-in amplifier located inside the innermost shell is used to detect the voltage appearing across the inductor. The reference signal for this phase detector derived directly from the voltage on the outer charged shell is phase shifted continuously at a linear rate of 720°/h ($\varphi' = \omega' t$) and sent inside on a light beam. The optical photons are passed through a small pin hole which does not pass the rf for which the hole constitutes a waveguide below cutoff frequency. The output of the lock-in is amplified, sent through a voltage-to-frequency converter, and then returned outside the shells on another light beam. This output frequency is detected and counted for 50-sec intervals. Finally this digital output is Fourier analyzed for the phase-shifting frequency $\omega'$.

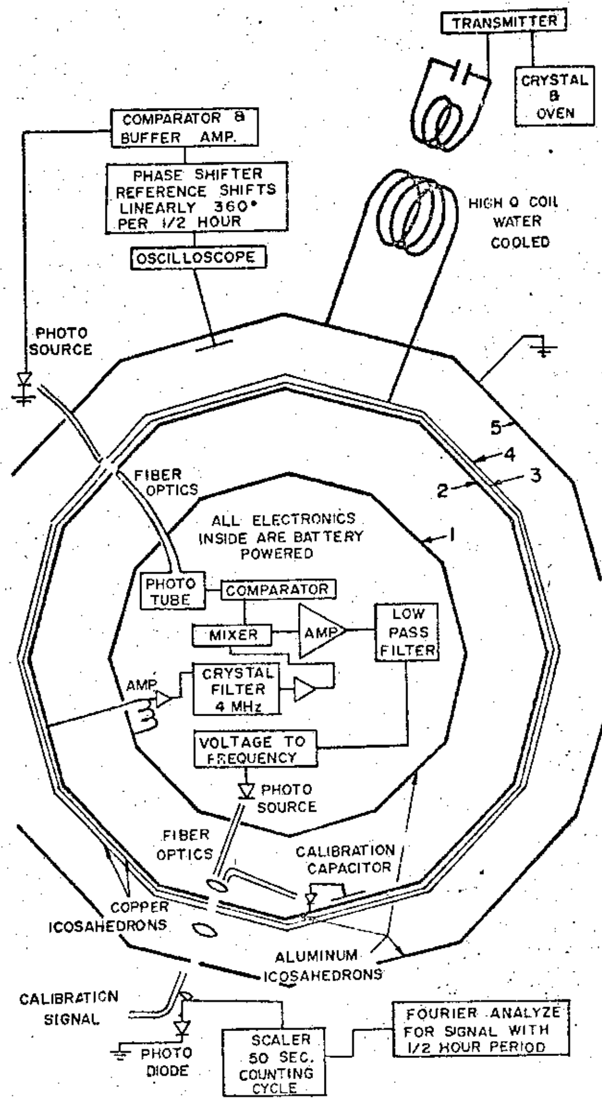


FIG. 1. Schematic drawing of the apparatus. A 4-MHz voltage is applied between shells 4 and 5. A signal of less than $10^{-12}$ V could have been detected between shells 1 and 2.

Stray electric and magnetic fields are shielded against by the skin effect in the conducting shells which attenuates them exponentially with the distance of perpendicular penetration into the conductor. In practice this shielding is limited not

Table I. Results of various tests of Coulomb's law and tests for a nonzero photon rest mass.

| | Coulomb's Law violation of form $r^{2+q}$ q | $\mu^2 = (\frac{m_0 c}{\hbar})^2$ | Photon rest mass $m_0$ |
|---|---|---|---|
| Cavendish (1773) | $2 \times 10^{-2}$ | | |
| Coulomb (1785) | $4 \times 10^{-2}$ | | |
| Maxwell (1873) | $4.9 \times 10^{-5}$ | | |
| Plimpton and Lawton (1936) | $2.0 \times 10^{-9}$ | $1.0 \times 10^{-12}\ cm^{-2}$ | $\leq 3.4 \times 10^{-44}\ g$ |
| Cochran and Franken (1967) | $9.2 \times 10^{-12}$ | $7.3 \times 10^{-15}\ cm^{-2}$ | $\leq 3 \times 10^{-45}\ g$ |
| Bartlett, Goldhagen, Phillips (1970) | $1.3 \times 10^{-13}$ | $1 \times 10^{-16}\ cm^{-2}$ | $\leq 3 \times 10^{-46}\ g$ |
| Williams, Faller, Hill | $(2.7 \pm 3.1) \times 10^{-16}$ | $(1.04 \pm 1.2) \times 10^{-19}\ cm^{-2}$ | $\leq 1.6 \times 10^{-47}\ g$ |
| Schroedinger (1943) | Test of Ampere's Law from Geomagnetic Data | $3 \times 10^{-19}\ cm^{-2}$ | $\sim 2 \times 10^{-47}\ g$ |
| Gintsburg (1963) | | $5 \times 10^{-20}\ cm^{-2}$ | $\leq 8 \times 10^{-48}\ g$ |
| Nieto and Goldhaber (1968) | | $1.3 \times 10^{-20}\ cm^{-2}$ | $\leq 4 \times 10^{-48}\ g$ |
| Feinberg (1969) [a] | Dispersion of light | $8 \times 10^{-14}\ cm^{-2}$ | $10^{-44}\ g$ |

[a]Feinberg, Ref. 12.

And though in terms of a rest mass it does not quite equal the sensitivity which has been obtained from the most recent analysis[8,14] of Earth's geomagnetic data, as a laboratory test it has the advantage that all of the experimental parameters are controlled and can be individually tested and varied. A very sensitive test using an *LCR* circuit has just recently been reported[15]; however, the authors themselves express reservations about the precise interpretation of their results.

The theoretical implications of a nonzero photon rest mass if one were found to exist would be considerable. In addition, the existence of a finite rest mass could have some practical importance when describing the magnetic field of large bodies such as Jupiter and the sun.[7] A further extension of the limit on the rest mass via laboratory tests of Coulomb's law though increasingly difficult is experimentally feasible. In view of a number of experimental unknowns which preclude much if any extension of this limit using Schrödinger's method, the greatest promise for further improving on the rest mass limit may well rest with precision tests of Coulomb's law. To this end, continuing effort on Cavendish-type experiments would appear to be worthwhile.

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